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THE SEARCH FOR SAFETY

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SOCIAL STRATEGY AND TACTICS
IN THE SEARCH FOR SAFETY

In The Search for Safety, Aaron Wildavsky argues that the pervasiveness of uncertainty, the complexity of economy and society, and the trial-and-error strengths of a competitive market system make alleviating damages through market processes superior to a preventive strategy of regulatory anticipation. Close examination of a number of issues that are central to his argument — anticipation vs. resiliency, the effects of wealth on health, the competitive market as an engine for social trial and error, and equity in aggregating benefits and costs across individuals and different probabilistic states — leads to considerable skepticism as to whether Wildavsky's strategy of less government regulation and a more unfettered market would indeed produce greater safety.

The late Aaron Wildavsky's *Searching for Safety* (New Brunswick, N.J.: Transaction, 1988) is a bold, multifaceted statement about how to improve safety and health. Speaking from the vantage point of a period of unexampled technological change, it confronts the view that such change and the economic processes in which it is embedded are responsible for generating grave social hazards even as they allay other, possibly less grave, threats. If, as this view holds, technology is a mixed and increasingly dangerous blessing, then careful governmental regulation may be needed to encourage the positive

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Jerome Rothenberg, Massachusetts Institute of Technology, Department of Economics, E52-355, Cambridge, MA 02139, telephone (617) 253-2674, telefax (617) 253-1330, coauthor of *The Maze of Urban Housing Markets* (Chicago, 1991), is grateful for useful probing and advice from Jeffrey Friedman.

aspects and discourage the negative aspects of technological dynamism.

Wildavsky's case rests on an unconventional, highly imaginative structure of intricate arguments in favor of continued technological change in a largely unfettered capitalistic market context. The keystone of the argument is that safety is improved by deliberately creating and taking risks that build up general resources which can be used to combat hazards as they actually emerge over time. Threats to health and safety are best handled by ongoing trial and error rather than by extensive efforts to anticipate and prevent dangers from appearing. The latter kinds of effort are generally misguided failures that only rob society of the resources that would impart the social resiliency needed to meet a wide array of unpredictable hazards.

Wildavsky's argument is deliberately provocative, paradoxical, against-the-grain, and, while shaded to some extent, sufficiently sharp in profile to generate uncomfortable new questions as well as reformulations of more familiar ones. It forces a deep reconsideration of a host of ongoing issues, many of which are connected in this work for the first time. In other words, whether or not one agrees with much or even little in Wildavsky's book, it performs a very valuable function in prodding us to consider difficult concerns in novel connections. My treatment of the book is in the spirit of using Wildavsky's special vision to help reexamine a number of urgent issues in the health, safety, and environmental areas.

It is only fair to state at the outset that while a number of Wildavsky's emphases seem to me most appropriate, I believe that his overall position has significant shortcomings. Nonetheless, even its inadequacies force us to grapple more searchingly with some issues that already seemed settled, and others that never appeared to be connected until Wildavsky showed them to be.

Before describing and evaluating Wildavsky's specific arguments, I should like to commend strongly two pervasive emphases in the book. One is the unifying concept of opportunity cost. Every private and social action has a cost if only in that the same limited resources could have been used differently. The net benefits of the alternative uses of a resource are among the costs of a given use of it. In the field of safety, where so many hazards clamor for attention, a careful attempt to set coherent priorities is critical. This

requires explicit attention to the opportunity costs of different remedial efforts.

The other emphasis is that most public and private actions have, or can have, complex and sometimes unfortunate repercussions through the market, through government, and through society. A full evaluation of any type of action must pay heed to the possibility of, and to the probable character of, such unanticipated consequences.

Wildavsky stresses these two considerations in many parts of his argument. They are essential to comprehending the complexity of the various arguments he deploys in this stimulating book.

Wildavsky's General Argument

Within the realm of health and safety, Wildavsky adduces three basic principles: the Principle of Uncertainty, the Axiom of Connectedness, and the Rule of Sacrifice. The first holds that irreducible uncertainty is universal; the second, that good and bad are intertwined in the same acts and objects; and the third, that "the safety or macro-stability of the whole [is] dependent upon the risk or instability of the parts." Together, these three generalizations imply that uncertainty cannot be eliminated nor damage avoided "in this realm" (4). If parts of the system are prevented from facing risks, the whole will become unable to adapt to new dangers. "Since there is no way of avoiding harm for everyone, the search for safety has to proceed on the understanding that the rule of sacrifice is inexorable: there can be no stable whole without some unstable parts" (5-6).

So the search for safety inherently involves risk. Moreover, since "safety must be discovered, and cannot be merely chosen," Wildavsky argues, "trial-and-error risk-taking, rather than risk aversion, is the preferable strategy for securing safety." Encouraging trial and error promotes resilience—learning from adversity how to do better—while restricting it tends to lock in existing hazards. Increasing the pool of general resources, such as wealth and knowledge, secures safety for more people than does using up resources in a vain effort to protect against uncertain, hypothetical dangers. Wealth adds to health (2), or as Wildavsky put it elsewhere, richer is safer:

Safety is a product of the accumulation of general resources (wealth, knowledge, energy, communication) that can be converted into what we need when we need it. . . . How, then, do we interrogate the unknown so as to have a greater probability of detecting and, if we fail, of remedying these threats to our health? Trying to predict or guess is unlikely to succeed. Who can remember anything important . . . that has ever been predicted? Worse still, huge expenditures on preventive measures are likely to be mistaken, costly and counter-productive. . . . The best process known to mankind for using our mistakes to do better is the decentralized, trial-and-error system of people coordinating their own efforts called capitalism.¹

Thus, Wildavsky argues that when regulating the development of medical drugs, if "we care about people, we [should] not respond in terms of prevailing policy: trial without error (no trials permitted without prior guarantees against damage); rather, we [should] choose the criterion of net benefit: more good than bad. Requiring that products do no harm to anyone means not only great cost, huge companies, large bureaucracies and less innovation, but also worse health."²

Does Wildavsky think regulation is ever warranted? He writes that only "when we know what bad thing will occur and the probability of its occurrence, and when the available solution leaves us better off than the present problem, [is] trying to prevent harm . . . desirable." But, he adds,

these conditions—know what, know when, know how—are onerous. Absent such ability—*most of the time*—health and safety are best achieved by trial-and-error risk-taking, not trial-without-error risk avoidance. Were the standard of proving no harm applied to existing activities as they are to new ones, no hospital in the world could stay open. We go to hospitals . . . because we rightly expect the good to outweigh the bad effects on our health. . . . Since the good and bad health effects of our products are intertwined in the same objects, . . . there is no way to improve health without incurring risks. . . . Fewer risks, fewer damages, but even fewer benefits leave us with worse health. (5, emphasis added)

The foregoing is a broad statement of Wildavsky's general viewpoint. He buttresses the three basic principles with examples and analogies of varied degrees of specificity and directness that create a network of much more specific detailed propositions illustrated with empirical examples. There is, however, no attempt to demonstrate or prove either principles or derivative propositions by rigorous logic or conclusive empirical analysis.

Wildavsky's makes a number of important claims: damage-prevention regulations are too prevalent; uncertainty should be a paramount consideration because of its pervasiveness; all processes have positive and negative effects; adaptation after damage is preferable to preventive measures; regulation must be based on adequate prediction; the criterion of appropriate action—public and private—is some aggregation of *net*, not gross, benefit; trial and error is the best way to adapt to hazard; the capitalistic market system is the best way to apply trial and error socially; general, not hazard-specific, resources are the best safeguard for safety; more wealth means more safety via a larger pool of general resources; preventive regulation lowers safety by depleting the stock of general resources in favor of ineffectual specialized resource use.

Uncertainty and Social Policy; Anticipation vs. Resilience

The central question for Wildavsky is whether or not the government should intervene in market processes, either in the development of specific means of countering present hazards, or in the prevention of expected future hazards. The effect of the first form of intervention is to discourage private market-driven innovations that would have been more effective than intervention. The effect of the second form of intervention is to waste resources, because the anticipated hazard fails to materialize, or because preventing even hazards that turn out to be real costs resources that would otherwise be devoted to producing consumption or investment goods, or because regulation discourages privately initiated technological change and disrupts otherwise efficient production patterns in various unanticipated ways.

The pervasiveness of uncertainty is central to Wildavsky's description of these policy choices: uncertainty increases the likelihood of misguided interventions that are poorly targeted or have significant unexpected side effects, including poorly understood or poorly anticipated economic disruptions that may well ramify in complicated, far-reaching ways. "Predicting wrongly, when these 'guesstimates' are backed up by the force of the state, wreaks havoc upon innocents: lives are disrupted, jobs lost, anxiety increased, and resources diverted from more productive and hence safer uses." Since Wildavsky assumes that "the most likely eventuality by far is

that most of the low probability events will not happen, and that whatever does happen will be unexpected, i.e., it will not be among the envisioned possibilities at all," he concludes that "we might be better off, in fact, when faced with a large number of low-probability dangers to increase our generalized capacity to respond to *realized* risks than to dissipate our strength by advance preventive efforts to ward off all the hypothetical hazards that a rich imagination might generate" (91, emphasis added).

For Wildavsky the alternative to intervention in anticipation of damage is the alleviation of damage after the fact, which is facilitated by the availability of "general resources": "wealth, knowledge, energy, communication."³ If these resources are basically produced by the private sector, then public intervention that diminishes, distorts, or disrupts the private sector in the name of safety will likely hurt safety in the long run.

Evidently, Wildavsky assumes that the uncertainty that renders anticipating government action wasteful and ineffective will not also diminish the usefulness of after-the-fact damage alleviation. How can we account for this asymmetry in Wildavsky's treatment of uncertainty?⁴ One obvious interpretation is that anticipation, being forward looking, is always faced with uncertain outcomes, while alleviation, mobilized after the fact, completely dispenses with uncertainty. This is certainly true, but it raises the central issue: on balance, does society gain or lose if it attempts to prevent some disaster but, because of faulty predictions, is only partially successful? Or is it better to wait for disaster to happen, and then attempt to mitigate some of the resulting damages? Here the issue of uncertainty is *defined* so that it applies only to the anticipation strategy. If prediction is so wide of the mark that no actual prevention occurs, then surely the alleviation strategy is better because it wastes no resources trying to prevent a non-event, while spending only as much to mitigate what damage does occur as would have had to be spent anyway.

But anticipatory efforts are not made in a vacuum; they are tried where there is presumptive cause-and-effect evidence, or experience, to suggest that if particular kinds of cautionary actions are not taken, unfortunate events may well occur. Preventive actions may not be practicable for events far in the future, but they may be useful, for example, in putting in place monitoring systems and

teams to predict the course of a hurricane days in advance, together with evacuation plans; or in inoculating groups vulnerable to a spreading epidemic; or in instituting regulations to keep workplace gases below a level known to have deleterious effects with constant exposure. These are anticipatory actions that we *have reason to expect* will in fact avert probable danger.

Preventive efforts will vary, depending on the bases for prediction, the probability of success, and in the consequences of partial failure. These prospects for partial success must be balanced against the alternative of *no success whatever*, which is what occurs if we wait for disaster to strike.

In what way, then, do general economic resources alleviate damages? Alleviation means that the human costs of an event are determined in conjunction with *what is subsequently done about the event*. After-the-fact coping may lead to adverse consequences, but Wildavsky argues that general wealth, particularly the stock of information, energy, and communication, can significantly influence the degree to which these human costs can be mitigated. Information, energy, and communication resources can facilitate the physical movement of resources, and wealth enhances the discretionary resources to be mobilized. In some kinds of totally unexpected catastrophes this may indeed be the best that society can do. But even so, it may be woefully inadequate. While the enormously developed molecular biology establishment created over the last 40 years has not sufficed to conquer or even control the AIDS epidemic, what mobilization of effort has occurred on this front is not primarily a product of the competitive market but of vast amounts of government-financed basic research.

Where adverse events can be predicted to some extent, it is not at all obvious that the general level of ex post resources available for mobilization will mitigate damages enough to outweigh even partial preventive action, especially when we balance, against the excess costs of the latter due to poor anticipatory aim, the excess costs of the former due to the need to cope with much greater damages than would have occurred had preventive efforts taken place. This is an *empirical* question that depends on the ability to predict hazards; it is not the all-or-nothing matter sometimes suggested by Wildavsky. Similarly, the question of how much mitigation of damage can be accomplished after it has occurred must be an-

swered differently depending on the kind of damage involved. For example, preventing lung cancer by taking statistically justified precautionary action against smoking seems far more efficient than attempting to treat the disease after it occurs, despite the enormous medical capacity that, as with AIDS treatment, has been accumulated and is waiting for mobilization.

Thus, the uncertainty attending anticipatory efforts does not automatically make alleviation the better strategy. The vast differences in the predictability of various hazards, and the fact that only limited success is possible in undoing damage that has already occurred, make Wildavsky's preference for the latter strategy questionable at best. Far more empirical study is needed before a persuasive verdict is possible.

The choice between anticipation and alleviation, while central to Wildavsky's overall thesis, is not the same as the choice between using government or the competitive market to implement either strategy, although there is some connection between the two issues. Wildavsky generally associates anticipation with government regulation of the economy, and alleviation with private market action. But sometimes he associates government action with alleviation and market action with various forms of anticipatory behavior. Since much of the practical weight of Wildavsky's book is that there is too much government regulation because a competitive market can achieve a better balance between anticipation and alleviation, *and* implement each of these strategies better, a few words are appropriate on this auxiliary issue.

Anticipatory actions emerge from the competitive market where saleable commodities are concerned. Perceptibly safer versions of a commodity, or commodities that can protect users against predictable hazards, will be profitable and hence likely to be produced through competitive pressures. Even some forms of precautionary information—for example, safety ratings of consumer goods—will be generated by the market. But these will be inadequate where product performance is hard to monitor by users, where hazards are not widely or accurately perceived, or where people do not realize that they are uninformed. Even more important in the present context are the many occasions when hazards are not attached to proprietary commodities—that is to say, where they result from externalities. Here producers are insulated from market pressures for safety, so that the profit motive

is not geared toward controlling hazards. Many forms of regulatory action are directed against just this kind of phenomenon. Finally, there are natural disasters that are immune either to prevention or rectification by the consumption of products sold in private markets.

It is sometimes argued that capitalism can overcome such problems through insurance, which can make up for anticipatory failures. While insurance policies may not prevent damage, they may offset damages that have already occurred.⁵

Several remarks should be made. First, while insurance policies compensate for risks after damage has occurred, the terms of insurance contracts are based on solid anticipatory information: probability distributions of the different possible outcomes of specific processes. Without actuarial experience by which to predict outcomes there is rarely private insurance (Lloyds of London is a notable, and rare, partial exception). So insurance is really a mixture of anticipatory and alleviative elements.

Second, some forms of predictable hazards, such as floods, find only sparse instances of private insurance—because of the large scale of potential liabilities. In these instances, governmental supplementation is often necessary.

Third, most anticipatory regulations are attempts to prevent or mitigate potential disasters before they occur. Purchasing insurance is an anticipatory action taken not to avert or change the magnitude of a disaster but merely to change the distribution of the damages among the population after the disasters have occurred. That a policyholder is compensated for loss of his home by an insurance fund does not mean that no damage has been sustained: the cost to society is registered in the liability of the insurance fund. Redistribution of sustained losses can have important mitigating effects on the welfare losses of the population as a whole, but this effect is—as almost always with alleviation—only *partial*.

Insurance certainly is not inherently superior to the imperfect avoidance achievable with anticipation. Our previous questions about the empirical comparison between anticipation and alleviation apply as well to insurance. Moreover, the anticipatory propensities of the competitive market seem imperfect in their own right, and do not obviate the special potentialities of government anticipatory actions—despite the imperfections of the latter.

Is Wealth Safety?

Another question of empirical magnitudes is that of the cost of safety regulations in terms of their discouragement of private innovations that could alleviate or better avoid relevant damages (a direct cost) and their diminution of overall economic activity, diversity, competition, growth, and hence the economy's ability to alleviate damages (an indirect or opportunity cost). Wildavsky exaggerates the size of both types of cost.

Wildavsky's argument for the direct costs of safety regulations relies on the view that private innovations that provide safety are highly sensitive to government intervention. Regulations scare away such innovations or make them highly unprofitable. It is true that heavy-handed, direct command-and-control regulations can choke off private initiative, sharply raising costs and the prospects of future profit-killing regulations. But subtler forms of regulation, such as performance standards and, even more so, market-type regulations (charges, fees, penalties), operate simply by changing *relative* incentives. That is, they discourage private agents from making socially more dangerous changes while encouraging socially less dangerous ones. Indeed, regulatory systems of this sort can increase the urgency of positive change precisely by making the status quo less profitable, thereby *increasing the profitability of private innovation*. In this way, anticipatory government intervention can act as a spur to anticipatory private innovation.

Thus, when Wildavsky criticizes anticipatory, command-and-control nuclear safety regulations on the ground that they actually produce less safety, not more, he cannot properly be thought to have undermined all forms of regulation. In this particular case, it is the application of a linear model of safety to a technology characterized by complex nonlinearities that is at fault. His argument establishes only that regulations should recognize the complexity of the whole system being regulated; he does not show that we must rely on unregulated resiliency.

Thus, the direct costs of anticipatory intervention, in the form of less innovation, could, through proper regulatory techniques, be reduced to quite tolerably low levels. Regulation would ideally consist in imposing differential charges on various private products and production processes that generate specific kinds of foreseeable damages. Such charges should be based on the best available infor-

mation, with the dependability of the predictions taken into account. The passage of time would allow corrections of these charges, some being dropped entirely, others being added.

By contrast, in analyzing the indirect costs of anticipatory intervention, we must recognize that only modest amounts of resources are diverted from the economy as a whole to prevent or repair specific risks, causing at most a marginal diminution of economic activity. The average participant in the economy is minutely "taxed" in order to alleviate significant welfare losses suffered by those exposed to various risks. Preventing the loss of a lung or a limb or the contraction of a debilitating disease is surely a benefit that is so concentrated that it outweighs these highly diffused costs.

It follows from Wildavsky's strategy of resiliency that any government intervention that inhibits the market's efficiency in producing income, by diminishing the size of the general resource stock, makes a society not only poorer but less safe. And since Wildavsky believes that the hazards that emerge in reality are almost completely different from those that were expected, he contends that there are virtually no benefits from anticipatory safety regulations to weigh against the economic-cum-safety costs.

Were Wildavsky correct about the relationship between a society's general resource stock and its level of safety, one should expect economic indices to correlate well with measures of health. Yet the association of per-capita income with health indices is quite rough. It differs for different indicators. Moreover, there is considerable variance in the association. Some poor countries, such as China, do much better than their socioeconomic peers in this regard. Some rich countries, such as the United States, do much worse than their peers. Moreover, while groups of rich nations do better on the average than poorer groups, there is no dependable year-by-year association for any one country between changes in per-capita income and changes in health. Wildavsky's favorite example of the effects of socialism on health, the USSR, actually showed that health may *worsen* even while per-capita income slowly grows.⁶

Closer examination of the specific circumstances of individual countries makes clear that health depends on many particular features that are only weakly, or even negatively, associated with total income or wealth — such as the *distribution* of wealth, accessibility to medical care, nutrition, housing, public-health programs, liquor and cigarette consumption, and cultural practices. In this context,

the extremely modest decrements in per-capita income that might be associated with safety regulatory programs (were Wildavsky's argument valid) would have at most a negligible effect on aggregate health indicators.

The health effects would be greatest if the income decrements mostly affected the poor and deprived portions of the population. But given the complexity of the market repercussions of safety regulations, such an effect cannot be established. In fact, the effect of safety regulations may well be exactly the opposite, with most of the protection they accord likely to fall on the lowest income groups, which are more unprotected from workplace and environmental hazards. By protecting them from injuries that keep them off the job, health and safety regulations might also raise the standard of living of the poor—and without adversely affecting the health of wealthier groups, given their comfortable existence above the health thresholds. Thus, anticipatory regulations might improve health both directly *and* through the “wealth is health” mechanism that Wildavsky emphasizes.

The Market as a Process of Trial and Error

Exactly how does Wildavsky envision wealth as translating into safety? Once resources have accumulated, how can they be mobilized to alleviate hazards after the fact? Wildavsky's answer is that we need the ability to combine general resources with know-how derived from extensive experience with hazardous processes. For Wildavsky, competitive capitalism is a superb institution for bringing about this combination of resources and knowledge. Products that are found wanting from a health and safety point of view will be improved under the working of basic market incentives: improvement is required for continued marketability. Products whose primary purpose is to cure illnesses or increase safety will be under competitive pressures to do this primary job better or lose market share. So competitive pressures will promote a vast set of trial-and-error searches for greater safety and health by market participants. Market success and failure will both demonstrate and promote safety improvements over time.

This process should, above all, be an incremental one, ensuring that adaptations are practicable and irreversible calamities avoided.

Market processes achieve these goals because they rely on a large number of small participants, no one of which generates large consequences.

Certainly, errors are made in the market. But as Wildavsky argues, without trials and their attendant errors there is no progress. What is important is that damages produced by errors not be too serious to be corrected and that there be continuing pressure toward such correction. Trial-and-error processes alone are of limited social usefulness where there are large-scale irreversible outcomes, and where there are serious obstacles to repeating trials. Wildavsky agrees with these qualifications but believes that market processes generally avoid them because of the multiplicity of actors and actions. In fact, this multiplicity does not ensure that markets will effectively perform the socially useful trial-and-error function.

First, some outcomes can be quite large to the individuals affected by them, even though large numbers of individuals may not be affected in a given instance. A variety of irreversible negative effects on health (e.g., crippling and fatal injuries) flow regularly from the market.

Second, competitive pressures to correct errors occur only if those who suffer because of them are adequately informed as to the nature of the damage, its source, and the possibility of avoiding it. Producers have no inherent interest in avoiding damages to their customers when the latter do not recognize the damage or know where it came from or how to avoid it. In these cases they will not voluntarily seek out such information and there will be no profit in providing it. Where damages are acute and immediate and their source obvious, consumers will tend to be informed as a matter of course. But there are many circumstances where these conditions will not prevail. The resulting consumer ignorance could easily remove any incentives for producers to correct errors.

Market processes do provide a partial corrective to consumer ignorance where hazard identification is relatively straightforward. When consumers suspect they are dangerously uninformed about private commodities, they may be willing to purchase product information as though it were a private commodity itself. Entrepreneurs may then profit from selling such information, their success depending on the perceived relevance and accuracy of the information they provide. *Consumer Reports* and *The Medical Letter* are notable examples of such success. But such instances are in fact

rare. The more likely sources of such precautionary information are governmental product safety programs, scientific studies, and journalistic exposés.

Market provision of information is especially unlikely where hazard identification is not straightforward. Some hazards have a long latency period. Some are only occasionally triggered by exposure to a risky product or are the outcome, after a long period, of exposure to a combination of causative factors. Sometimes tracing causation is difficult: the route from producer to product to consumer can be a long and complicated one. Some products incorporate several inputs at different stages of production. Pinpointing the source of the hazard is often difficult, but this does not make the damage any less serious. Sometimes, even where accurate responsibility could theoretically be assigned, it may be obscured by proprietary information that is inaccessible to consumers. And, by definition, damages due to externalities, even where they are understood, fall outside the coordinating reach of the market altogether. Neighborhood effects and the whole set of hazards that diminish environmental quality offer no direct commercial recourse to their victims.

These factors constitute a set of serious qualifications to the ability of a competitive market system to generate self-correcting trial and error. The structural weakness involved is not that market processes generate products and operations that are risky; risk is pervasive, in various kinds and degrees. The problem is that markets often provide inadequate, inaccurate, or misleading information that prevents people from making the best tradeoffs between the risks and the advantages of various products and jobs.

The market's myriad decentralized actions do not themselves ensure adequate safety. Centralized controls of various sorts are needed. These have been instituted in the form of regulations, constraints, information programs, licensing and certification (where complex producer skills are largely indescribable to consumers), and damage liability.

In principle, of course, licensing and certification, like some product information, could be provided privately by market processes. But the likelihood of success here is smaller than even in the case of product information. This is because licensing and certification deal with services, not tangible products separable from their producers. The kinds of services dealt with involve high stakes for

consumers—a possibility of major, irreversible, and even fatal outcomes. Since the services are inseparable from their practitioners, adequate investigation for certification or licensure would typically require serious invasions of privacy and proprietary secrecy. While government can legitimately represent the public interest to justify such violations, private investigators would have no such legal warranty. Besides this, suspicions about biased, collusive private investigations for profit would damage the credibility of market licensing efforts. Government certification is certainly subject to corruption also, but there is a presumption that legal requirements for openness make this corruption easier to discover. In the private sector such collusion is not criminal, and it is protected by the privacy of private property. Where the hazards consequent upon misrepresentation are high—as is typical—these weaknesses of private responsibility make government action more attractive, despite visible evidences of its imperfection.

Wildavsky recognizes the need for some government intervention but claims that at present regulations are too numerous, too onerous, and generate unwarranted biases. He is especially concerned with what he claims is a serious asymmetry in the regulatory treatment of old versus new products, and with the stifling effects of product liability law as currently practiced.

Wildavsky claims that with old products the market is permitted to operate unfettered, and it therefore freely carries on its effective self-correcting process. The burden of proof is on government if it wishes to impose standards on or otherwise constrain established market actions and products. This protects against unwarranted market interventions and serves to ensure that whatever interventions occur do, at least in prospect, improve overall safety. Not so for new products. In the United States, new products in health- and safety-sensitive areas (e.g., new drugs) require *advance* licensing, obtained through extensive pre-trial screening. While old products are subjected to trial and error, new products are forced to undergo “trial without error” (34), a bias against innovation that reduces social safety.

Wildavsky contends that this bias stems both from an inadequate appreciation of the strengths of allowing errors to occur and from a failure to consider that screening new products produces considerable opportunity costs. To prohibit all risky new products associated with even small degrees of risk means banning many that

would do much more to abet safety—say, by saving the terminally ill—than to impair it. Moreover, screening promising products seriously delays their introduction and so needlessly allows damages to persist that could have been alleviated earlier. Legislators indulge this bias because, while old risks are attached to goods whose overt benefits are known to the public, the potential benefits attached to new goods are not widely known (35).

Wildavsky is probably correct in his view of the political psychology of the asymmetry, but he overlooks the connection between pre-use screening and trial and error. Trial and error is a process that is supposed to maximize the effectiveness of inadequate information. Use what you have and see what happens. This try-and-interpret-the-results procedure can take many different forms. The least effective forms use trials so loosely structured that little can be dependably inferred from their outcomes. At the other extreme is the controlled experiment. This maximizes the amount of dependable interpretation that can be extracted from limited information. When health and safety are at risk and the risk of irreversible and even fatal damages is high, it seems most appropriate that as much as possible be made of the current state of scientific knowledge. Screening requirements embody exactly this principle. Uncontrolled consumer use without such screening would not only represent high risk—given, say, the high safety-efficacy failure rate for new drugs—but would be a wasteful way to use currently inadequate knowledge.

If resources were unlimited, this argument would point toward the selective screening of suspicious old products as well. The enormous number of old products makes general screening impracticable. Moreover, their apparent safety over extended use makes the probability of catching hazards out of random sampling quite low. But screening for limited groups of old products whose history of use raises doubts might well be an appropriate strategy.

Wildavsky argues that “trial without risk” means ignoring the *net* benefits of a new product. Rejecting an efficacious new drug because it has serious side effects can represent a social loss if the side effects are outweighed by the good the drug may do. But Wildavsky assumes that regulation merely stops, deters, or discourages private actions rather than redirecting or channeling them. We referred to this assumption above in the context of innovation in general. Here, it means that Wildavsky fails to allow that

screening may steer competitive energies toward new avenues of improvement, rather than simply stopping innovation. The often-repeated tendency of pharmaceutical companies to continue searching for targeted products that meet safety standards, despite many early failures, illustrates how regulation does not necessarily curb innovation. The consequence of rejecting an effective new substance is more likely to be that a *better* one is soon developed than that no new substance at all is introduced. Screening can be a process of learning, rather than of suppression.

Moreover, Wildavsky is wrong in arguing that in the United States, the Food and Drug Administration basically adheres to a no-risk criterion for the acceptance of new drugs (224). The FDA actually approves many preparations that are only incompletely efficacious and have a variety of side effects. All preparations for cancer, AIDS, hepatitis, Parkinson's disease, muscular dystrophy — indeed, for all currently incurable diseases — that have been approved by the FDA are both incompletely efficacious and have a variety of side effects, some nearly as serious as the illness being treated. Physicians daily must struggle with the balance between partial benefits and serious side effects in deciding which drugs to prescribe.

Much the same criticism can be leveled against Wildavsky's complaints about unnecessary delays in introducing drugs that are accepted. "More lives are lost from delayed introduction of beneficial new drugs than would be lost even by another tragedy as devastating as thalidomide," he writes (192). But if one does not know how to recognize "beneficial" new drugs dependably without screening, and if the purpose of screening, with all its delays, is to point the way to devising new drugs that are *more* beneficial, then introducing "beneficial" new drugs without screening has a possibly serious indirect opportunity cost — namely, the delayed development and introduction of *even more* beneficial drugs — in addition to the direct cost of unnecessarily high side effects.⁷

The net weight of both sets of direct and indirect costs is not easy to gauge empirically, so Wildavsky makes his case with examples rather than solid quantitative evidence. How pervasive are well-publicized cases of apparently unwarranted delay? We certainly do not have quantitative estimates of the damage that has been directly prevented by screening, and indirectly by the incentives screening creates. More extensive, tightly analyzed data are required before

we can make a trustworthy judgment about the issue. Wildavsky's anecdotal evidence is insufficient.

Wildavsky also deplores American product liability law. He writes:

One rationale for product liability is that producers of unsafe goods should be forced to internalize the cost of the hazards they inflict on society. Companies will compensate people hurt by their products, and the costs will in turn be spread to all customers. Fewer dangerous products will be purchased, and people will be safer. This analysis ignores the fact that society does not compensate companies for every life their products have saved. Penalizing them for every life that is lost thus creates a disincentive to accept small risks that reduce larger risks. If the company should pay, say one million dollars for every life lost, then they should be paid the same per life saved; but they are not. This asymmetry is clear in the vaccine industry. If companies must limit themselves to designing products that are not only risk-reducing for most people but also risk-free for all, then the incentive to reduce already existing risks through trial-and-error will diminish sharply.

Of course, we do not claim that companies ought to be compensated for the lives their products save. Our argument is that a standard of negligence is preferable to one of strict liability; for when it is necessary to prove negligence, this imbalance between penalties for lives lost without rewards for lives saved would not exist. (178)

Wildavsky goes on to criticize the current doctrine of "joint and several liability" as one that badly unbalances equity and generates seriously perverse resource-use incentives.

It is true that current practice in product liability and tort law in general has created chaotic incentives for resource use, not only by private companies but by governments and nonprofit organizations as well. But Wildavsky's justification of product liability reform demands close scrutiny despite the validity of his conclusions.

If products that may fail without egregious negligence are clearly and accurately labeled with warnings against their dangers, then consumers' decisions to buy some of them and not others can be said to represent deliberate tradeoff between risks and benefits. Under these circumstances, product breakdowns that are not due to negligence should not be actionable at law, because consumers are getting what they contracted for. Products that delivered benefits at a lower price in money and risk would presumably be rewarded by increased sales. The more important the imbalance between products' different risks and benefits, the greater would be

the increase in market popularity of safer products at the expense of their less safe competitors.

However, in the absence of explicit, detailed warnings about safety lapses, the same degree of consumer awareness and therefore of implicit informed consent is missing. Under these conditions, product lapses unavoidably suggest that consumers did not actually get what they "bargained for," and imposing seller liability is more reasonable. Yet, such a liability could carry appropriate producer incentives: under strict liability, a perceptibly safer car stands to gain new customers for its seller as well as the avoidance of liability penalty. Still, to make sure that customer responsiveness to differential safety is not overwhelmed by direct liability penalties, the apparent trend toward unreasonably high liability awards must be reformed.

Risk Aggregation and Equity

As an advocate of maximizing net rather than gross benefits, Wildavsky places much-needed emphasis on the requirement that we subtract costs—usually neglected *opportunity* costs—from our perception of the benefits of regulation. Despite the importance of doing so, however, "adding up the pluses and minuses" is not a neutral process. Biases occur in the way we aggregate costs and distribute benefits across both individuals and probabilities.

Without saying so explicitly, Wildavsky suggests that the aggregation of different kinds of outcome among different kinds of people under different risk conditions should be carried out so as to obtain gains in total expected net benefits. This is indeed the conventional form of risk aggregation. Very serious measurement problems are often encountered here, especially where comparable measurement across different categories is attempted in a single monetary dimension. Wildavsky does not make such an attempt, but his policy positions based on net benefits suggest this kind of aggregation. In any case, his intention is less important for our purposes than the opportunity to clarify some of the problems such an approach would encounter.

The problems with aggregating across persons are dramatic. In speaking about the economic costs of regulatory programs above, we suggested that what was often at stake is an attempt via regula-

tion to prevent very serious damage to a relatively small group at the expense of a highly diffused pattern of decreased welfare to a very large group, such as "consumers." To prevent fatal or catastrophic illnesses or injuries rather than waiting for these human costs to be inflicted on a relative few may require extracting marginal losses from a much larger population. Aggregating the economic costs might suggest that such regulations are unwarranted even though they prevent the destruction of some people's lives at a much less devastating cost to many others.

Not only is it possible to underplay the disastrous effects of concentrating serious harm on a few people to avoid spreading a larger aggregate cost in very diffuse form over many, but one may ignore the fact that those harmed may be the *innocent, involuntary victims* of the actions of others. Individuals injured because of the consequences of risks they knowingly took have an equity standing different from those whose injuries were involuntarily, even unknowingly, received.

These people have been dealt with unfairly — unjustly. The regulatory state should often be seen, then, as an attempt to prevent injustice. While this is not as true of the regulation of hazards that are well known and are incurred voluntarily, the pervasiveness of some risks, the absence of good alternatives, and a lack of information about them may create situations in which regulation may achieve the widely held desire to avoid injustice. We already spend considerable resources toward more conventional manifestations of this desire.

Not only bureaucratic but judicial regulation can be seen as aiming toward equity. Both strict liability and the search for "deep pockets" legitimated by the joint and several liability doctrine of tort law aim at assisting impoverished victims who cannot afford their injuries by shifting the cost to richer corporations that can. (Wildavsky criticizes them for precisely this reason.) That at least the second of these equity-oriented tactics brings in its wake highly detrimental incentive effects on resource use should be a warning that distributional arguments have to be very carefully considered before they are applied, because they may generate considerable offsetting social costs that vary in size depending on how they are implemented.

The second problematic dimension of net benefit aggregation is adding up different risk situations. Mathematical expectation is the

conventional procedure for doing so, although the last 20 years have seen increasing criticism of this procedure and the use of a variety of alternative methods. Under mathematical expectation, the net benefits of each possible outcome are multiplied by their "probability" and these probability-weighted net benefits are added up across all possible outcomes. The choice criterion then used is to select the action with the highest expected total value of net benefits. In this spirit, Wildavsky argues that to choose to guard against low-probability, high-consequence events, instead of choosing to guard against much more probable events that create low to moderate damage, will lead to much "excess damage" (87). He especially excoriates the use of "worst case scenarios" as an extreme example of what happens when the spirit of maximizing expected value is violated: worst-case scenarios exaggerate hazards "thousands of times over" (28). Supercaution is socially irrational.

The unease that some of us who engage in social valuation feel about strictly maximizing expected value stems partly from the earlier point that money measurement is especially misleading where *different kinds of damage* are involved: how many headaches equal blindness or the loss of a leg? But the unease also stems from general attitudes about how choices should be made in the presence of *different kinds of uncertainty*. When confronted by the probabilities of the set of possible outcomes of a given course of action (a probability distribution), the conventional method is to summarize the overall probability-weighted mean value (the expected value). But where two or more actions with the same expected value have very different distributions, it may not be enough to know the mean value. It may be relevant to know also that one action has a tiny probability of generating a cataclysmic disaster, while the other can lead only to a very high probability of modest damages. Other features, like the range of positive and negative outcomes, may also be useful to know. Moreover, an expected value is most relevant where we can expect many repeated trials of the action. Does this empirical relevance remain when only a single trial is involved?

Moreover, when we translate outcomes in physical terms into the actual behavior of choosing or rejecting different gambles, we can be interpreted as displaying different degrees of caution—as, for example, when we both buy insurance and gamble. These co-existing attitudes are relevant to our evaluation of different strategies for anticipatory public actions.

One way of describing genuine differences in how we make choices when faced with uncertainty is to refer to our basic optimism or pessimism: do we prefer to try to achieve great gains that have little chance of materializing or to avoid highly unlikely catastrophes? Do we mind the possibility that we may be overreacting to small chances of serious danger (crying wolf on the basis of sensors that confuse wolves with dogs), or do we prefer to gamble (crying wolf only when the animal is close enough to make no mistake whatever about its identity)?

Our choice between these polar positions, of course, derives to some extent from our past experience with the consequences of this kind of choice. We are likely to differ about both probabilities and the relative importance of these different kinds of reaction error. Wildavsky repeatedly makes it clear that he is an optimist in both regards. His critics generally, and the present author, are considerably less optimistic.

Searching for Safety is an important book. Many of our judgments and actions about health and safety risks are the result of habit, or incrementalism, or the hot sense of urgency, or the inability even to attempt to make a coherent, consistent whole of vastly disparate commitments. Wildavsky has issued a ringing, impassioned challenge to the field, and has raised many penetrating questions about established ways of thinking about risk and preparing for it. That there are difficulties with several of his arguments is less important than that they force us to rethink and recommit ourselves. It is clear that there are no easy answers here. There are not even easy questions. We should be deeply indebted to Wildavsky for forcing us to search for difficult answers by asking tough questions.

NOTES

1. Aaron Wildavsky, "The Secret of Safety Lies in Danger," *Society* (November/December 1989): 4.
2. *Ibid.*
3. *Ibid.*
4. I wish to thank Jeffrey Friedman for help to clarify my argument on this issue.
5. I am indebted to Jeffrey Friedman, private communication, for this suggestion.
6. Victor Fuchs, *Who Shall Live?* (New York: Basic Books, 1974).
7. For this side of the argument see Peter Temin, *Taking Your Medicine: Drug Regulation in the U.S.* (Cambridge, Mass.: Harvard University Press, 1980).

SELF-INTEREST AND RESPONSIVE REGULATION

To make regulation more responsive to the public interest, Ian Ayres and John Braithwaite recommend improved administrative techniques, such as enforcement pyramids, to improve industry compliance; and they advocate vesting public interest groups with regulatory powers, so as to reduce regulatory capture by industry. Their arguments, while suggestive, do not take seriously enough the subtle and multifarious influences of interest groups. Consequently, the authors' recommendations are not likely to improve regulation's responsiveness to the general welfare.

In *Responsive Regulation: Transcending the Deregulation Debate* (New York: Oxford University Press, 1992), Ian Ayres and John Braithwaite have set about to improve regulation. They are well qualified to do so. Ayres is one of that growing number of scholars who are bringing economic theory to bear on the analysis of law. He has the further distinction of applying to regulation a relatively new and highly regarded branch of economics—game theory. Braithwaite has conducted in-depth regulatory studies of the pharmaceutical, coal-mining, and nursing-home industries. The authors propose, as indicated by their subtitle, to “transcend the intellectual stalemate between those who favor strong state regulation of busi-

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Jack High, Visiting Professor of Business Administration, 277 Morgan Hall, Harvard Business School, Soldiers Field Road, Boston, MA, 02163, telephone (617) 495-6408, the editor of *Regulation* (Michigan, 1991), thanks Jeffrey Friedman for many worthwhile suggestions.

ness and those who advocate deregulation" (3). The proximate cause of the stalemate is dissatisfaction with regulation's performance. "On the right, critics claim that regulation is an inefficient and stifling force that drives them to laissez-faire prescriptions. The political left, however, often views contemporary regulation as ineffectual or coopted. The resulting crisis in confidence leads these camps to strongly divergent proposals for reform. This book is an attempt to transcend the stalemate by proposing alternative modes of regulation that take each criticism seriously" (158).

Traditionally, the case for government regulation has rested on the theory of "market failure," which means the failure of the market to live up to the theoretically inspired idea of equating prices and marginal costs across all occupations and industries. By this standard, markets commonly fail. Large firms, for example, are often cited for charging prices above costs, although IBM has recently reminded us that large firms can also raise costs above prices. A divergence between price and cost may also occur if there are "external effects," another oft-cited example of market failure. A coal-burning manufacturer does not figure into its calculations the costs it imposes on others by fouling the atmosphere. Parents do not figure into their calculations the benefits accruing to the community from educating their children. By prohibiting monopoly, limiting pollution, providing education, and so on, the government can bring costs and benefits into alignment.

For many years economists advocated various kinds of regulation on these theoretical grounds. Such advocacy cut across ideological boundaries. Milton Friedman, for example, argued for government financing of schools, and George Stigler praised the Sherman Act. Friedman and Stigler were perhaps more cautious about arguing for regulation than others, but the standard of efficiency set up by welfare economics was taken seriously by both sides of the political spectrum.¹

In the 1960s, a fundamentally different view of regulation began to take hold in the ranks of the academy. Beginning with two influential works by Gabriel Kolko, *The Triumph of Conservatism* and *Railroads and Regulation*, scholars noticed that regulation often served the interests of regulated groups. Kolko argued that in railroads, banking, telecommunications, and other industries, business firms of the Progressive Era sought regulation because it benefited their bottom line. Economists, led by such luminaries as James Buchanan, Sam

Peltzman, George Stigler, and Gordon Tullock, formulated a theory that conformed to the accumulating evidence that regulation serves special interests. According to this theory, regulation is an exchange between politicians and various groups that stand to benefit.²

Responsive Regulation attempts to bridge the public- and private-interest theories of regulation. It aims to correct for market failure without falling prey to government failure. Most of Ayres and Braithwaite's ideas for improving regulation depend in one way or another on game theory. This theory has made substantial headway in economics over the last decade, especially in the field of industrial organization, which includes the study of regulation. The theory of games is mathematical, and it often does not take long for the theory to pass beyond the ken of ordinary mortals. Fortunately, Ayres and Braithwaite keep their theory comprehensible. Their mathematics does not go beyond basic algebra and calculus. With some effort the reader, even if not particularly numerate, can follow their main ideas.

Ayres and Braithwaite apply game theory to regulation by using the prisoners' dilemma, a staple of game theory, which considers the situation of two suspects apprehended by the law. Each of the culprits, call them Bonnie and Clyde, is offered the following deal. If Bonnie confesses and Clyde does not, Bonnie goes free and Clyde does twenty years behind bars. If Bonnie does not confess, but Clyde does, Bonnie gets the twenty years, and Clyde goes free. If Bonnie confesses, and Clyde does too, they each do ten years. If neither confesses, they each do two years. Each has a temptation to confess, but each, too, has an incentive to stand on the fifth amendment and remain silent.

Ayres and Braithwaite apply this dilemma to regulation by postulating a similar situation between a regulated government agency and a regulated firm. Each has two options. The agency can either cooperate with the firm or punish it; the firm can either comply with regulation or evade it. Each round of this game can then have four results, depending on the options exercised by the agency and the firm: 1) the agency cooperates and the firm complies; 2) the agency cooperates and the firm evades; 3) the agency punishes and the firm complies; 4) the agency punishes and the firm evades. Associated with each pair of options is a regulatory outcome.

For concreteness, Ayres and Braithwaite use the example of pollution control and postulate the following outcomes. For option pair 1, where the agency cooperates with the firm and the firm complies

with regulation, 100 tons of pollution are removed from the air at a cost of \$2 million to the firm. This is the cooperative solution. For option pair 4, where the firm evades the regulation and the agency initiates legal action, 75 tons of pollution are removed from the air at a cost of \$3 million to the firm. This is the noncooperative solution. For option pair 2, where the agency cooperates but the firm evades regulatory rules, 50 tons of pollution are removed from the air at a cost of \$1 million. In this solution, the agency is either suckered into cooperating, or else captured by the firm. For option pair 3, where the firm complies but the agency punishes, 125 tons of pollution are removed from the air at a cost of \$4 million to the firm. In this solution, the firm is suckered into complying, when it would have been better off to evade.

Under these assumptions, we can ask how the regulatory agency and the firm will behave. Consider the agency. The agency can reduce pollution the most, 125 tons, if it decides to punish the firm while the firm decides to cooperate. The large amount of pollution that can be removed by the agency's adopting a hostile attitude to the firm tempts the regulators to be tough. But there is a counteracting force at work. If the firm knows the agency is going to be hostile, managers will be better off evading the regulation, thus reducing the firm's costs from \$4 million to \$3 million. But then the agency will only succeed in reducing pollution by 75 tons, which is even less than the 100 tons of pollution that can be removed if both the agency and firm work together. Therefore, if the agency believes that its actions will induce noncompliance, it might be better off cooperating. But the emphasis is on the "might be": if the agency cooperates and the firm evades, only 50 tons of pollution will be removed. Given these considerations, what should the agency do?

The firm faces the same kind of decision. It is cheapest for the firm to evade pollution controls while the agency takes a cooperative attitude. But the firm knows that if it evades, the agency has an incentive to adopt a hostile attitude; by so doing the agency can further reduce pollution. And if the agency adopts a hostile attitude, it will cost the firm even more than it would had both entities cooperated. What should the firm do?

If this game is played only once, there are no definite answers as to what the firm and agency should do. But if the game is played repeatedly, month after month, year after year, then there is a plausible answer. Robert Axelrod, in studies of a similarly structured

game, has found that it is usually best to play a “tit-for-tat” strategy, whereby a player starts off cooperating, and continues to cooperate as long as her opponent does so. However, if the opponent ceases to cooperate, then so does the player. Furthermore, the player refuses to cooperate as long as her opponent does. However, as soon as the opponent switches back to cooperative moves, so does the player. Over the long haul, this is the strategy that will usually bring a player the greatest return.³

A prominent lesson that Ayres and Braithwaite draw from this game-theoretic conception of regulation is that there are gains to be had from cooperation between regulating agencies and regulated firms. Much of *Responsive Regulation* is devoted to methods by which regulating agencies can secure compliance from firms. One of the methods they offer is what they call an “enforcement pyramid.” At the bottom of the pyramid lies persuasion, which is regarded as the cheapest form of regulation available to the agency, because it avoids costly litigation and inspection procedures. As we move up the pyramid, enforcement becomes more severe and more costly, and eventually includes criminal penalties and the revocation of a firm’s right to do business. Ayres and Braithwaite argue that regulatory agencies should obtain compliance by starting at the bottom of the pyramid and working their way up. This is a kind of tit-for-tat strategy. It has the advantage of cooperating with those firms that intend to comply and punishing those that do not. It also has the advantage of enabling the agency to distinguish between degrees of noncompliance. A small infraction can be met with a mild retaliation (35–38).

As a means of governance, the enforcement pyramid seems sensible enough. Informal pyramids of this kind are commonly employed, for example, in the management of a firm. An employee is hired with the expectation that he will adhere to the rules of the firm, and he is treated accordingly—management works cooperatively with the employee. If the worker violates the rules or disrupts production, management can retaliate by firing him, which would represent the peak of the pyramid. Of course, this would not always be an appropriate response. A manager might not want to fire a worker who is generally productive but who occasionally shows up late. A fine or some other lesser punishment would permit the firm to retain the productive services of the worker, yet discourage unwanted behavior. The enforcement pyramid is a governance procedure, widely used in commerce and the law, applied to regulatory regimes.

A related but broader idea, that of a "pyramid of enforcement strategies," is advanced as a schema for regulating various industries. At the bottom of this pyramid is self-regulation; at the top is command regulation with nondiscretionary punishment. By analogy to the first pyramid, the range of enforcement strategies is supposed to induce cooperation. By imposing the threat of stringent regulation, an agency signals to firms in an industry that they will be better off regulating themselves. Self-regulation, in which the agency negotiates the goals of regulation with the firms but then leaves it up to the firms to set and enforce the rules that will best achieve those goals, is a cheap and effective means of regulation, when it works. The threat of meddling oversight with stiff, mandatory punishment will make it work more often. This is the message of Ayres and Braithwaite. However, if self-regulation does not work, the agency need not resort immediately to draconian measures. It can move up the pyramid with techniques like enforced self-regulation, a situation in which the regulating agency sets the goals and rules for the firms, but then leaves it up to the firms to police themselves and report to the agency.

The idea behind the pyramids, and behind proposals like enforced self-regulation, is to get regulation to work better. The authors want to reduce the red tape and sluggishness of existing regulation. They want to induce cooperation between the regulators and the regulated. They want to lower the administrative and legal costs of regulation. Their presumption is that, under most circumstances, such improvements in regulation will improve social welfare. It is precisely at this point, however, that the economic theory of regulation rears its troublesome head. Improved cooperation between regulator and regulated need not, and in general will not, improve social welfare. Take as an example the Interstate Commerce Act of 1887. This Act created an independent commission that was charged with looking out for the public interest by curbing the abuses of the railroads. For many years politicians, the public, and most scholars of regulation assumed that the Interstate Commerce Commission did just that. Then in 1963, Kolko demonstrated that many railroads had favored regulation and were instrumental in securing its passage. In 1965 Paul MacAvoy documented that trunk lines of New England were able to charge higher rates when regulated than they had when market forces determined rates. Here was presumptive evidence that, behind the public-interest rhetoric of regulation, the commission had acted as an agent to secure cartel prices for the railroads.⁴

Because of cozy arrangements between regulators and interest groups, increasing cooperation can work against the general welfare, and not merely because consumers are charged higher prices. Regulation introduces various kinds of distortions into the market process. The performance of the regulated airline industry in the United States illustrates these distortions. Under regulation, rates were set by the Civil Aeronautics Board. These rates were high; it cost almost twice as much to travel on an interstate, regulated flight as it did to take a comparable intrastate, unregulated flight in California or Texas. The airlines could not compete by lowering prices, so they competed by providing costly services to the flying public. They provided the air traveler plenty of capacity by investing heavily in large, fuel-guzzling airplanes, such as the Boeing 747. They provided convenience by flying often and by instituting a great many direct flights from point to point. They provided amenities in abundance. Richard H. K. Vietor writes, "When American [Airlines] installed piano bars, TWA countered with electronic draw-poker machines. Live entertainment proliferated, with musicians, magicians, wine tasters, and Playboy Bunnies." The high price of flights, the investment in inefficient aircraft, and the oversupply of service occurred in a cooperative environment between the airline industry and its regulators. During the hearings that Senator Edward Kennedy held on airline regulation, the groups that most strongly opposed deregulation were the airline executives and unions.⁵

If the railroads and airlines were isolated industries, there would be little to worry about in Ayres and Braithwaite's proposals. However, cooperation between business and government often produces regulation that, in Stigler's words, "is acquired by the industry and is designed and operated for its benefit." Samuel Insull of Chicago Edison lobbied for regulation of the electric utilities so that he could exclude competitors. So did Theodore Vail of American Telephone and Telegraph. Edmund Taylor, maker of Old Taylor Kentucky Bourbon, worked closely with Harvey Washington Wiley, the father of America's pure food law. In return, Wiley demanded that Taylor's competitors be forced to label their product "imitation whiskey." Even "social" regulation of safety and the environment has been subject to the influence of special interests. Eastern coal companies lobbied vigorously for the mandatory installation of scrubbers so that utility companies would not use the low-sulphur coal found in the west. Rust-belt firms lobbied for safety regulations that

would impose proportionately higher costs on their rivals in the sun belt.⁶

The insight that regulation serves interest groups led not only to the development of theory, but also to the political movement to deregulate; which, as Ayres and Braithwaite properly point out, took place mostly in the 1970s, not during the Reagan era. Just as the belief that regulation served the public interest was espoused across the political spectrum, so was the proposition that it served special interests. The movement to deregulate airlines was spearheaded by Edward Kennedy and supported by Jimmy Carter. The historical evidence on the Progressive Era was presented by Gabriel Kolko, working from a Marxist point of view. The early development of the economic theory was at the hands of the Chicago School—Buchanan, Peltzman, Stigler, and Tullock.

It is instructive to inquire into the means by which the economic theory of regulation is played down in *Responsive Regulation*. Ayres and Braithwaite are too sophisticated, too knowledgeable about regulation, not to realize that the regulatory agencies can be persuaded to serve the interests of interest groups, and we will return to consider their method of discouraging the capture of regulation by business. But the issue here is not really capture; it is that the regulatory process, from design to passage to implementation, is a mutually beneficial exchange between politicians, regulating agencies, and private-interest groups. Regulation exists because it enables particular groups to obtain, through the coercive powers of government, ends they cannot obtain through market mechanisms. When regulation is modeled as a prisoners' dilemma game, in which the cooperative outcome serves the public interest, this essential insight of economic theory is slighted.

Consider the pollution matrix used by Ayres and Braithwaite. The cooperative outcome of that matrix removes 100 tons of pollution from the air at a cost of \$2 million to the firm. This is assumed to be in the public interest. What assurance do we have that the cooperative solution will produce a proper result, rather than producing too much or too little pollution? Does the political process by which regulation is passed and implemented produce the information required for affected parties to accurately judge its effects? Does the process put in place a mechanism by which we can determine if the right decision has been made? Does it assign responsibility in such a way that proper decisions are rewarded and improper decisions are

punished? Does it ensure that pollution will be removed by cost-effective means? When we construct a matrix in which the cooperative solution to a game assumes that the public interest is being served, we are implicitly answering yes to these questions. The economic theory of regulation does not allow us to implicitly assume that regulation produces results in the public interest; it requires that we examine both the actual forces that drive the regulatory process and the outcomes of those forces. All too often, regulation has not answered these questions properly. Instead, behind a thick fog of public-interest rhetoric, special-interest groups have grown rich and particular politicians have grown powerful, mostly through cooperation with one another, and mostly at the expense of an uninformed and unsuspecting public.

The interest-group theory of regulation, while maintaining that regulation often serves private interests, also recognizes the possibility that it might advance the common weal, especially in those situations where private property is not well defined. Clean air is an example this. It is a common good and subject to the familiar tragedy of the commons. Because there are no property rights in air, it will tend to be overpolluted, just as land will tend to be overgrazed or fish will tend to be overharvested if there are no property rights in land or water. What we want from our judicial or legislative processes are ways to establish property rights in air, or methods to approximate the results of private property. It may be that game theory, or some other theoretical technique, is able to help in the solution of these problems. We can hope that Ayres and Braithwaite, with their wide knowledge of regulatory experience and their familiarity with game theory, will take up the problems encountered in generating accurate information, in arriving at proper decisions, and in holding regulators accountable for mistakes. These problems have priority, both theoretically and practically, over modeling regulation as a game in which these difficult problems are assumed to have been solved to the general good.

I said earlier that Ayres and Braithwaite have considered the capture of regulatory enforcement by businessmen. Their solution is what they call "tripartism." It calls for bestowing regulatory powers on various public-interest groups. An interest group would have the same legal right as agencies to hold hearings, gain access to information, and decide cases. The reason for granting interest groups such power is to raise the costs of capture; in order to capture regulation,

not only would firms need to buy off the regulating agency, they would need to buy off all empowered interest groups as well.

Although this idea is put forth as a new and fairly radical departure from current enforcement practice, it amounts to expanding the size of the regulating agencies and changing the voting rules necessary to make enforcement decisions. For example, Ayres and Braithwaite say that if several public-interest groups are "given standing to enforce the law, a firm will only be able to achieve zero-sum or inefficient capture if it captures the *PIG*'s [public interest group's] individual payoff as well as the agency's." However, this assumes that each public-interest group will have a vote in regulatory decisions, and that unanimity is required. If there are, say, 10 commissioners and three public-interest groups that have votes, and the voting rule is a simple majority, firms may not need to pay attention to public-interest groups. They might achieve their ends by influencing the seven commission members who are easiest to persuade. In general, of course, increasing the number of persons who decide regulatory issues will increase the costs of capture, but this will be true whether or not the additional voting members represent public-interest groups.⁷

From the point of view of interest-group theory, another and more important concern with tripartism is the selection of the interest groups that would be empowered. Ayres and Braithwaite "assume that it is possible to identify" public-interest groups "whose mission is the same as that embodied in a regulatory statute: environmental groups for environmental statutes, animal welfare groups for animal welfare statutes, civil liberties groups for privacy statutes, women's groups for affirmative action legislation, and so on." The assumption here is that legislators pass laws that serve the public interest, but that once those laws are turned over to the bureaucracy to enforce, special interests capture them. But of course interest groups play a substantial role in the passage of the laws. Legislators introduce and support laws that appeal to powerful constituencies who will support their re-election. These constituencies, who often oppose one another, structure legislation, as far as possible, to their advantage. The advantage of these constituencies is the actual the mission of regulation, even though the stated mission is almost always couched in terms of public interest.⁸

If we accept this insight of the interest-group theory, we cannot be sanguine about tripartism. To the extent that the empowerment

of public-interest groups gives regulatory agencies independence from legislators, it has little chance of being adopted. Tripartism would be tantamount to politicians and special-interest groups acting against their own interests. Even if it were adopted, tripartism would not be likely to have the effect intended by Ayers and Braithwaite. If regulation results from a combination of particular interest groups, and if the powers-that-be decide which public-interest groups will be empowered, then it follows that the choice of groups will generally be those that advance the interests of the ruling coalition. The process will select against those groups that would genuinely serve the general interests of society.

I have focused on the game-theoretic approach to regulation taken by Ayres and Braithwaite, and on the main shortcoming of that approach—its failure to adequately address the economic theory of regulation. This focus has not done justice to many of the contributions that Ayres and Braithwaite have made to regulation, especially their discussion of enforced self-regulation and their views on the psychological and sociological aspects of regulation. These contributions, while impressive in their own right, will improve the effects of regulation only if we can address the central difficulty of the regulatory state—its ability to cloak special favors under the guise of public interest.

NOTES

1. Standard references to the theory of market failure include Francis Bator, "The Theory of Market Failure," *Quarterly Journal of Economics* 72 (August 1958); Frederick M. Scherer, *Industrial Market Structure and Economic Performance* (Chicago: Rand McNally, 1970), 8–38. Tyler Cowen, *The Theory of Market Failure: A Critical Appraisal* (Fairfax, Va.: George Mason University Press, 1988), is a collection of the classic articles on market failure and various criticisms of the theory.
2. Gabriel Kolko, *The Triumph of Conservatism* (New York: The Free Press, 1963); idem, *Railroads and Regulation* (Princeton: Princeton University Press, 1965). The modern theory, often referred to as the economic theory of regulation, is a part of public choice theory, which applies economics to the study of government. Classic works in public choice theory include Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957); James Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962); William Niskanen, *Bureaucracy and Representative Government* (Chicago: Aldine-Atherton Press, 1971). The seminal works in the

- theory of regulation are George Stigler, "The Theory of Economic Regulation," *Bell Journal of Economics and Management* 2 (Spring 1971): James Buchanan and Gordon Tullock, "Polluter's Profits and Political Response: Direct Controls versus Taxes," *American Economic Review* 65 (March 1975): 139-47; Sam Peltzman, "Toward a More General Theory of Regulation," *Journal of Law and Economics* 19 (August 1976): 211-40; Gary M. Becker, "A Theory of Competition among Pressure Groups for Political Influence," *Quarterly Journal of Economics* 98 (August 1983): 371-400.
3. Robert Axelrod, *The Evolution of Cooperation* (New York: Basic Books, 1984). Axelrod conducted two tournaments in which contestants tried out solutions to a prisoners' dilemma game. The tit-for-tat strategy won both tournaments, one of which had 14 players, the other of which had 63. However, it is important to note that tit-for-tat does not always win, and that in pair-wise comparisons against tit-for-tat, other strategies do just as well. See Axelrod, 192-205.
 4. Kolko, *Railroads and Regulation*; Paul W. MacAvoy, *The Economic Effects of Regulation: The Trunk-line Railroad Cartels and the Interstate Commerce Commission before 1900* (Cambridge: MIT Press, 1965). For earlier discussions of capture theory, see Lee Benson, *Merchants, Farmers, and Railroads* (New York: Russell and Russell, 1955); and Mervin Bernstein, *Regulating Business by Independent Commission* (Princeton: Princeton University Press, 1955).
 5. Richard H. K. Vietor, "The Hubris of Regulated Competition: Airlines, 1925-1988," in Jack High, ed., *Regulation: Economic Theory and History* (Ann Arbor: University of Michigan Press, 1991), 31.
 6. For empirical studies that support the theory, besides Kolko, MacAvoy and Vietor, see Robert W. Crandall, *Controlling Industrial Pollution* (Washington, D.C.: Brookings Institution, 1983); Robert Poole, *Unnatural Monopoly* (Lexington, Mass.: Lexington Books, 1985); A. P. Bartel and L. G. Thomas, "Predation through Regulation," *Journal of Law and Economics* 30 (October 1987): 239-64; Donna J. Wood, *Strategic Uses of Public Policy* (Boston: Pitman, 1986); Jack High and Clayton Coppin, "Wiley and the Whiskey Industry," *Business History Review* 62 (Summer 1988): 286-309. For an overview of empirical evidence supporting the economic theory of regulation, see Robert Tollison, "Interest Groups and Regulation," in High, ed., 59-76.
 7. Ayres and Braithwaite do not discuss voting rules, which, it seems to me, bear importantly on their proposed reforms. The classic treatment of voting rules is Buchanan and Tullock, *The Calculus of Consent* ch. 5.
 8. On the relationships among constituencies, legislators, and regulatory bureaus, see Terry M. Moe, "The Politics of Bureaucratic Structure," in John E. Chubb and Paul E. Peterson, eds., *Can Government Govern?* (Washington, D.C.: Brookings Institution, 1989); B. R. Weingast and M. J. Moran, "Bureaucratic Discretion or Congressional Control? Regulatory Policymaking by the Federal Trade Commission," *Journal of Political Economy* 91 (October 1983): 765-800.

REGULATION AND THE "RIGHTS"
REVOLUTION: CAN (SHOULD) WE
RESCUE THE NEW DEAL?

In After the Rights Revolution, Cass Sunstein confronts the "Chicago" intellectual tradition on the effects of government regulation. While recognizing the many failures of regulation, Sunstein argues that most regulation serves a potentially useful purpose and that the courts can be relied on to correct the worst regulatory failures. He offers a dizzying array of prescriptions to ensure this outcome, many of which would appear to run afoul of clear congressional intent. In a more recent book, Breaking the Vicious Circle, Stephen Breyer offers a different diagnosis. He sees health-safety-environmental regulation as essentially out of control because of the "vicious circle" created by irrational and ineffective government responses to the public's concern about the risk of exposure to carcinogens. Breyer does not believe that this vicious circle of excessive and irrational regulation can be broken by the courts. Instead, he suggests the creation of an elite bureaucratic corps with the mission of proposing more effective and less costly approaches to controlling threats to human health.

It was perhaps forty years ago that economists and lawyers at the University of Chicago began to question the efficacy of govern-

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Robert W. Crandall, a Senior Fellow in the Economic Studies Program at the Brookings Institution, 1775 Massachusetts Avenue, NW, Washington, DC 20036, telephone (202) 797-6000, telefax (202) 797-6004, is the author of, *inter alia*, *After the Breakup: The U.S. Telecommunications Industry in a More Competitive Era* (Brookings Institution, 1991) and *Regulating the Automobile* (Brookings Institution, 1986).

ment regulation.¹ Study after study emerged from Chicago economists and their intellectual progeny that showed federal government regulation of surface transportation,² natural gas,³ airlines,⁴ communications,⁵ ocean shipping,⁶ securities trading,⁷ and electricity,⁸ ostensibly designed to protect consumers, actually reduced consumer welfare. More recently, Clifford Winston has shown that these economists correctly predicted the direction of the effects of *deregulation*, but—if anything—were likely to underpredict the economic benefits of such deregulation.⁹

Since 1960, federal regulation has increasingly turned to control of potentially damaging “externalities” from modern industrial processes—pollution, threats to human safety, exposure to dangerous carcinogens. And although the record on the effects of such regulatory exercises is still rather incomplete, there is an emerging consensus that this type of regulation has been excessive, pressed to levels at which the additional costs are likely to far exceed the additional benefits to human health, safety, or welfare.

In the face of this rather dismal evidence on the efficiency and efficacy of regulation, it is surprising indeed to find a new book from a member of the University of Chicago law and political science faculty that seeks to put a respectable face on the regulatory state. But that is what Cass Sunstein attempts in *After the Rights Revolution: Reconceiving the Regulatory State* (Cambridge, Mass.: Harvard University Press, 1990).

Completing the New Deal: The “Rights” Revolution

Sunstein begins with a tribute to Franklin Roosevelt’s “second Bill of Rights,” which provided the basis for a major growth in the federal government in the 1930s. Roosevelt attacked the Depression by invoking a variety of new “rights” for each American: the right to a fair wage, the right to decent housing, the right of farmers to fair prices for their crops, the right of retirees to a fair retirement income, the right to safe bank deposits, and so on. Dozens of new agencies, such as the National Labor Relations Board, the Social Security Administration, the Federal Housing Administration, and the Federal Deposit Insurance Corporation were established to attempt to secure these new rights.

In assessing Sunstein’s argument, however, we would do well to

remember that the problem Roosevelt inherited was not the willful wholesale denial of economic “rights,” but the inability of the economy to produce the housing, food, clothing, education, and other amenities to fulfill these rights. It is useful to recall that unemployment averaged more than 20 percent of the work force during Roosevelt’s first term. Between 1929 and 1939, there was no growth in the real income available to the average citizen, which languished at about 25 percent of today’s average income.

There was hardly much political demand for health, safety, and environmental regulation in such a bleak economic environment. Of course, economic growth eventually resumed with the outbreak of World War II, but one can hardly attribute this revival to the establishment of such economic regulatory institutions as the Federal Power Commission, the Civil Aeronautics Board, or the Federal Communications Commission—prime examples of Rooseveltian regulatory agencies. Indeed, we now know that many of these agencies were quite successful in restraining economic output, not in raising it.

It was only after economic rights were relatively secure due to the resurgence of the economy during World War II that the rights revolution began to address threats to human health and safety. In the 1950s and 1960s, Rachel Carson, Senator Estes Kefauver, and Ralph Nader began to campaign for a new federal regulatory push to protect workers, consumers, and various innocent species from the potential harm that results from modern economic activity. This type of regulation had largely been ignored by the New Deal, perhaps because its potential benefits were not valued very highly by millions of workers who were struggling for mere subsistence.

The new “rights” revolution, focusing on health and safety, was launched by changes to the Food and Drug Act (1962), by a new National Highway Transportation and Safety Act (1965), and by a major strengthening of the Mine Safety Act (1968). However, the revolution did not move into full swing until 1970 with the establishment of the Environmental Protection Agency (EPA), the Occupational Safety and Health Administration (OSHA), and the Consumer Product Safety Administration (CPSC). If Roosevelt was the father of the second Bill of Rights, Richard Nixon was at least partially responsible for the third.

A barrage of legislation accompanied and followed these events, thrusting the federal government into the regulation of environ-

ment, health, and safety in a manner that no one could have anticipated. Federal agencies now regulate the efficacy (as well as the safety) of new drugs, motor vehicle design, air quality, water quality, drinking water, solid waste, old hazardous waste sites, toxic substances, radiation, occupational safety, occupational exposures to hazardous substances, and the safety of toys.

These new federal health-safety-environmental ("social") regulatory programs have grown so rapidly that they have been a significant brake on private-sector productivity growth. Edward F. Denison found that pollution abatement expenditures alone reduced productivity growth by about 0.2 percentage points per year in the mid 1970s.¹⁰ More recent studies suggest an even greater impact through the 1980s.¹¹ Given these large effects on economic growth, each president since Lyndon Johnson has found it necessary to establish a central bureaucracy to attempt to review new proposed regulations and to discourage agencies from proceeding with those that are excessively costly for the protection they are likely to provide.

While the origins of federal social regulation extend back to the New Deal and even before, most of the current activity in this arena is decidedly different from that promoted by the New Deal, which was driven by a pressing need to stimulate the private economy. Social regulation clearly suppresses economic growth. Moreover, the federal agencies that have been established to carry out this new regulatory agenda are generally subject to the political control of the president. EPA, OSHA, and the National Highway Traffic Administration are not "independent" agencies of the New Deal variety, but generally part of a cabinet department and, therefore, much less insulated from politics.

The Origins of Regulation

Federal regulation began with the establishment of the Interstate Commerce Commission (1887), the antecedent to the Food and Drug Administration (1908), and the Federal Trade Commission (1914), to name a few prominent examples. Few of the pre-New Deal or New Deal regulatory agencies have performed very well if economic efficiency is the criterion. Most studies show that economic regulatory agencies have been particularly prone to restrict-

ing entry and raising prices to consumers while denying them the fruits of new technologies. Sam Peltzman,¹² George Stigler,¹³ and Richard Posner¹⁴ have provided us with the theoretical framework to understand this poor performance. In Sunstein's terminology, the problem is the prevalence of "crass private deals" that induce legislators to enact regulatory statutes—extracting resources from one group to favor another.

Vote-maximizing legislators may use regulation to cartelize taxi owners or hairdressers because the concentrated benefits delivered to producers will outweigh the political effects of the costs extracted from dispersed customers, who are unlikely to take political revenge on account of the loss of a few dollars apiece. Or legislators might vote to protect coal miners in the East from cheap Western coal in the name of "clean air," because the United Mine Workers will deliver votes (and election volunteers) to Eastern congressmen while Western congressmen barely feel the electoral impact from lower potential growth in their coal regions or in their energy-consuming industries. Both examples are rather typical of government regulation; neither strikes one as part of a "rights" revolution. Nor do such examples square with Sunstein's assertion that "social and economic regulation has been designed to promote economic efficiency" (228). Indeed, elsewhere he laments the fact that "regulatory legislation of the 1960s and 1970s has often been indifferent to cost. . . . No sensible regulatory program, however, can be indifferent to cost" (90).

The High Price of the Rights Revolution

The applied economics literature on regulation reveals that the regulatory component of the rights revolution has come at a high price. Winston has assembled the evidence on the enormous benefits that have accrued from dismantling a large part of the federal economic regulatory program, covering airlines, trucking, railroads, communications, finance, and energy.¹⁵ The most invasive and expensive forms of social regulation are still multiplying, however—implemented by OSHA, EPA, and the FDA. On the other hand, studies of OSHA's impact on worker safety show at best a minor beneficial impact.¹⁶ The ex ante agency estimates of the cost of successive OSHA and EPA carcinogen regulations now range up

to billions of dollars per cancer averted.¹⁷ Robert Hahn and John Hird conclude from a literature review that the estimated costs of environmental regulation range from \$55 billion to \$75 billion in 1988 dollars, while the benefits may be as low as \$16 billion or as high as \$136 billion, given the uncertainty surrounding the health effects of EPA regulation.¹⁸ FDA regulations have been trenchantly criticized for unduly delaying the introduction of new drugs.¹⁹

Even the most charitable interpretation of this literature would suggest that the "right" to health and safety has been costly indeed. Perhaps more chilling is the prospect that the new regulations authorized each year by existing federal statutes come at a higher and higher cost but deliver less and less additional protection from risk.²⁰

One might think that Sunstein has found some new mother lode of evidence that reverses decades of research into the causes and effects of regulation. But he has not. Rather, he simply ignores the most devastating contrary evidence, counters it with outdated studies, or finds ways to dismiss it as reflecting aberrant regulations that can be repaired by an enlightened federal judiciary. For example, Sunstein finds much to admire in environmental policy: "The United States faced an enormous problem of air and water pollution in the 1960s, posing a variety of short-term and long-term threats to safety and health. Largely as a result of environmental controls in the 1960s and 1970s, the problem has been substantially reduced" (77). He offers no citation to support either statement, undoubtedly because little scholarly research has documented the extent of pollution nor the effects of controls in limiting it. Sunstein then proceeds to note that "one study found annual health benefits in 1978 of \$5.1 billion to \$16 billion as the result of a 60 percent reduction in air pollution in urban areas with a total population of 150 million. These studies, now quite old, undoubtedly underestimate the benefits because they disregard the large gains since 1980" (77).

Once again Sunstein offers no evidence for the assertion of "the large gains since 1980." He does not mention that there are *no* sound estimates of the effect of regulation on air pollution levels for any period of recent history. Indeed, we do not even have accurate estimates of the levels of air pollution over time, much less the causes of any changes in these levels. Nor does he mention Myrick Freeman's study of the costs and benefits of air and water

pollution—virtually the only comprehensive analysis of the subject to date—which concludes that the total health benefits from all of these policies range from as little as \$5 billion to \$67 billion in 1984 dollars per year.²¹ A range of estimated health benefits this large can only reflect enormous uncertainty about the effects of these long-lived programs—an uncertainty not reflected in Sunstein's treatment of the subject. Moreover, Sunstein's assertion that much of the progress in pollution control attributable to emissions controls on motor vehicles is dubious.²² He offers (without attribution) estimates of the declines in individual pollutants between 1975 and 1984 or 1986, implying that these reductions are due in large part to regulation, not rising real energy prices (77). Even these data on emissions reductions are suspect: they are not based on direct measurement, but rather are very imprecise historical and current estimates produced by the EPA. Even the estimates of pollution in 1970 or 1980 may be changed annually by the EPA. We simply do not know how much progress has been made, and we surely do not know if regulation is responsible for any of it. Sunstein does not provide the reader with estimates of pollution reduction for the period since 1984 or 1986, perhaps because the rate of apparent progress has slowed measurably, the likely result of declining energy prices.²³

Perhaps more misleading is Sunstein's characterization of the efficiency of a wide range of health, safety, and environmental regulations. He cites approvingly the 1981 Graham and Vaupel study which found that 24 of 57 regulations cost less than \$300,000 per life saved, without noting that even this early study showed that most regulations involving carcinogens had costs far exceeding their benefits.²⁴ Had Sunstein cited the more recent work of John Morrall,²⁵ he would have left the reader with a different impression. To his credit, Sunstein admits in an appendix that the cost per life saved varies enormously across regulations, but he does not include in this appendix many of the most costly recent regulations.

Sunstein leaves us with the overall impression that regulation has generally protected us rather well at a fairly reasonable price. Sure, he argues, there are exceptions of bureaucratic overreaching, but these can be corrected by the appropriate judicial review.

Rights Revolution or a Vicious Circle?

My impression of the recent trend in regulation, on the other hand, more closely accords with that recently offered by Stephen Breyer. Breyer, Chief Judge of the U.S. Court of Appeals for the First Circuit, argues in *Breaking the Vicious Circle: Toward Effective Risk Regulation* (Cambridge, Mass.: Harvard University Press, 1993) that Congress, the regulators, and the public are caught in a vicious circle of regulation that leads to excessive expenditures on small risks and poorly chosen regulatory targets. Breyer fears that we have reached a "regulatory gridlock" in which we unwisely spend billions of dollars to regulate the "last ten percent" of health risk.

While Breyer is unwilling to scuttle regulation, the picture he paints of its impact is quite different from Sunstein's view. To Sunstein, many of the problems created by regulation reflect misunderstandings, "unanticipated systemic effects" (179), and "quite arguably perverse control" (182). To Breyer, the vicious circle that leads to overregulating minuscule risk is systemic. Breyer argues that the "tunnel vision" that drives regulators to require the expenditure of hundreds of billions of dollars in pursuit of the last 10 percent of each type of risk has led to a woeful misallocation of resources. This tunnel vision combines with an almost random selection of risks and inconsistency across regulatory agencies to create a crisis that requires immediate attention.

Breyer's diagnosis is that there is something fundamentally wrong with our society's approach to risk regulation, namely, that legislators and regulators respond much too sympathetically to every emotional demand for protection from risk, regardless of its validity or severity. Sunstein, on the other hand, gives the impression that it is not the fundamental approach of the regulatory agencies, but occasional aberrations in a societal pursuit of human rights that require correction. Their different diagnoses of the problem lead these two liberal law professors to prescribe very different cures for the disease of overzealous regulation.

Sunstein clearly understates the degree to which our regulatory institutions are misallocating resources, reducing living standards, and perhaps even *reducing* life expectancy in their most extreme actions.²⁶ While he apparently does understand the folly of much of what now passes for environmental, health, and safety regulation, he consistently fails to comprehend that the fault lies with Con-

gress, not with regulators' or the courts' interpretation of congressional statutes. Time after time, Congress passes regulatory statutes that *forbid* a comparison of the costs and benefits of alternative regulatory rules. Yet Sunstein argues that "courts should generally assume that Congress intends agencies to impose regulation after some kind of balancing of costs and benefits" (182), even though he admits earlier that it had no such intention (77). As justification for this prescription, he cites an appellate court opinion that concluded that the EPA is permitted to consider costs to some extent in setting clean-air standards. This hardly warrants the conclusion that Congress intended a *balancing* of costs and benefits.

Indeed, Congress has actively discouraged cost-benefit tradeoffs in regulating human health risks. Attempts to analyze regulations by the Office of Management and Budget under Reagan and Bush were severely curtailed by the Congress precisely because such analyses forced agencies to compare the costs and benefits of various alternatives before promulgating new rules affecting health and safety. Similarly, for more than a decade, pressure from Congress has forced the EPA to pursue a phaseout and outright ban of the remaining few uses of asbestos despite the enormous costs of doing so. Ironically, when the EPA finally proceeded to ban asbestos, its action was reversed in the courts because the relevant statute, the Toxic Substances Control Act, is the *only* environmental statute that actually requires a comparison of costs and benefits.²⁷

Because Sunstein seems to regard most regulatory statutes as benign reflections of the New Deal attempt to protect valuable rights, his only reform is to propose a set of "interpretive principles" that he would have the courts apply in reviewing agency actions. To this nonlawyer, these principles are at best baroque, if not incomprehensible. I have already commented on Sunstein's "principle" that courts should assume that Congress generally intends that benefits of regulations be balanced against their costs. Other "principles" include broad construction to protect "traditionally disadvantaged" groups, but narrow construction to counteract private-interest "deals"; the promotion of political accountability; protection against government failure due to "collective action problems"; an understanding of "systemic effects"; the avoidance of "irrationality and injustice"; and harmonization whenever any or all of the above principles conflict. But how would these principles be implemented? How could judges be ex-

pected to select from them when they conflict with one another? Sunstein's faith in the courts as an antidote to bad legislation is remarkable.

Breyer, a sitting judge, has no such illusions that the courts can break the vicious circle. He argues that "neither the courts nor Congress seem likely to provide real solutions to the problem of risk regulation." His solution, however, is to establish a new bureaucracy that could lay out the choices and identify rational strategies for coping with risky situations. This new organization within the executive branch—a successor to the various "regulatory review" bodies that have been established by every president since Richard Nixon—would have the authority and expertise to generate better decisions under existing statutes, and would even command sufficient respect to push Congress toward amending ill-advised statutes. Breyer believes that good intentions, hard work, and human insight can solve most problems. Some of us are not quite so optimistic.

Must We Vote Them Out?

I do not share Breyer's faith in governmental institutions, nor do I think that a superbureaucracy can or should be established that is at least partially insulated from political control. On the other hand, Sunstein's prescription strikes me as even more dangerous, for he would have judges with life tenure imposing a regulatory framework that Congress never intended and that bureaucratic experts never fashioned. The intent of Congress in passing statutes such as the Occupational Safety and Health Act or the Clean Air Act Amendments of 1990 may be to punish greedy capitalists for not reducing human risks to zero as part of a well-reasoned strategy to gain reelection.²⁸ To suggest that economic efficiency in risk reduction was intended by Congress is simply inconsistent with the plain language of these statutes.

If the political marketplace has failed in providing efficient government responses to health-safety risks, one might reasonably look for alternatives. Both Sunstein and Breyer suggest that these decisions be left with government but that those ultimately responsible for their design—either judges or superbureaucrats—be insulated from politics to some degree. Neither proposal seems satisfac-

tory for they provide no incentive system that would guarantee that resources would be used efficiently in controlling risk. A superbureaucracy run by Ralph Nader would surely not improve government risk regulation. And we cannot have judges sifting through mountains of technical economic and epidemiological evidence to instruct agencies where to make the tradeoffs between costs and benefits.

Before World War II, incumbent presidents were rewarded for increases in the size of old-style economic regulatory institutions, such as the Interstate Commerce Commission and the Civil Aeronautics Board. After 1948, voters apparently began to recognize that these regulatory commissions reduced their economic welfare, and they began to vote accordingly. Yet voters' support for incumbent presidents or their party since 1948 rises when they increase the size of health, safety, and environmental regulatory agencies.²⁹ One can only hope that this too will change when the evidence or the wastefulness of social regulation trickles out to the voters.

NOTES

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3. Paul W. MacAvoy, *Price Formation in Natural Gas Fields: A Study of Competition, Monopsony, and Regulation* (New Haven: Yale University Press, 1962).
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11. Michael Hazilla and Raymond J. Kopp, "Social Cost of Environmental Quality Regulations: A General Equilibrium Analysis," *Journal of Political Economy* 98, no. 4 (1990): 853-73; Wayne B. Gray and Ronald J. Shadbegian, "Environmental Regulation and Manufacturing Productivity at the Plant Level," Discussion Paper CES 93-6, Center for Economic Studies, U.S. Department of Commerce, March 1993.
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13. George J. Stigler, "The Theory of Economic Regulation," *Bell Journal of Economics and Management Science* 2 (Spring 1971): 3-21.
14. Richard A. Posner, "Taxation by Regulation," *Bell Journal of Economics and Management Science* 2 (Spring 1971): pp. 22-50.
15. Winston, 19, above.
16. W. Kip Viscusi, *Fatal Trade-offs: Public and Private Responsibilities for Risk* (New York: Oxford University Press, 1992), chs. 11 and 12.
17. John F. Morrall III, "A Review of the Record," *Regulation* (November/December 1986): 25-34.
18. Robert W. Hahn and John A. Hird, "The Costs and Benefits of Regulation: Review and Synthesis," *Yale Journal on Regulation* (Winter 1991): 233-78.
19. Henry G. Grabowski and John M. Vernon, *The Regulation of Pharmaceuticals: Balancing the Benefits and the Risks* (Washington, D.C.: American Enterprise Institute, 1983).
20. There is an upward trend in the agencies' own estimates of the prospective cost per life saved from new regulations. See *Budget of the United States Government*, Fiscal Year 1992, Part Two-370, Table C-2.
21. A. Myrick Freeman III, *Air and Water Pollution Control: A Benefit-Cost Analysis* (New York: Wiley, 1982).
22. Ibid.
23. The citation is U.S. Environmental Protection Agency, Office of Air Quality Planning and Standards, Technical Support Division, National Air Data Branch, *National Air Pollutant Emission Estimates*, annual editions.
24. John D. Graham and James W. Vaupel, "Value of a Life: What Difference Does It Make?" *Risk Analysis* 1 (1981): 89-95.
25. Ibid.
26. This is clearly the result of regulations that cost billions of dollars to prolong one statistical life. The effect of reduced income on life expectancy is well documented. See, e.g., the World Bank, *World Development Report* (New York: Oxford University Press, 1993).
27. *Corrosion Proof Fittings v. E.P.A.*, 947 F.2d 1201 (5th Cir. 1991).
28. As long as voters can be persuaded that the costs of regulation are borne by business, it is difficult for legislators to resist demands for tighter and tighter regulation regardless of its cost.
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OWNERSHIP CHANGE, CAPITAL ACCESS,
AND ECONOMIC GROWTH

Walter Adams and James W. Brock's Dangerous Pursuits offers conventional wisdom regarding the alleged evils of the corporate restructuring and financial innovations of the 1980s. Adams and Brock disregard how economic regulations enacted in the 1930s and 1940s led to passive investors and managerial control that furthered conglomerate acquisitions strategies of the 1960s and 1970s. This situation was undone in the 1980s with the rise of active investors and entrepreneurs who attempted to wrest control from established managers and companies so as to focus corporations on single lines of business, increasing efficiency and performance. The employment, productivity, consumer, and budgetary impacts of mergers and acquisitions were much more positive than Adams and Brock allow.

In the 1980s, bigness ceased to be an advantage in corporate America. For decades, corporate leaders had perceived as the preordained alignment of the economic universe the primacy of large, diversified, multidivisional, and increasingly concentrated industries. But now, the opening up of financial markets through credit liberalization and deregulation, new business strategies and technologies,

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Glenn Yago, Director of the Capital Markets Research Project for the Ph.D. Program in Economics at the City University of New York Graduate School and University Center, 33 West 42nd Street, New York, NY 10036, telefax (212) 382-1733, is visiting professor of economics at the CUNY Graduate School, professor of management at SUNY-Stony Brook's Harriman School for Management and Policy, and author of *The Decline of Transit* (Cambridge, 1985) and *Junk Bonds: How High Yield Securities Restructured Corporate America* (Oxford, 1991).

and expanded capital access and ownership have reinvented many a market and corporation. Innovations in credit analysis and capital availability made possible by the high-yield market and related developments in other markets broke the control over capital exercised by bureaucratic financial institutions and large corporations that had previously limited entrepreneurial growth.¹ Nonbank sources of capital (e.g., the high-yield market, securitization,² mutual funds, commercial paper, and the initial public offering market³) dramatically eclipsed the role of commercial banks. As a result of the wider availability of capital, the composition of corporate ownership changed rapidly.

Active financial investors had been driven out of corporations during the 1930s and 1940s with the passage of legislation such as the Glass-Steagal Banking Act of 1933, the Chandler Bankruptcy Revision Act of 1938, and the Investment Company Act of 1940. With these measures Congress limited the active participation of money managers and large owners of debt and equity on the boards of public corporations. Institutionally, this left corporate managers with greater autonomy and, increasingly, without monitoring by financial institutions or owners. As investors were forced into a more passive role, they came to depend on profiting from short-term arbitrage rather than long-term value generation.

In the 1960s and 1970s, managers who were afraid of being confined to their mature markets were freed by this situation to diversify wildly, leading to industrial conglomeration. Defense companies went into mass transit and nearly out of business, steel companies went into oil, tire companies went into gas, and oil companies bought circuses and insurance companies.

The conglomerate merger trends of the 1960s and 1970s were mitigated by value-oriented acquisition strategies (as opposed to defensively oriented ones) undertaken during the 1980s. In that decade investors purchased over \$6 billion in securities from companies in the growing fields of cable, cellular communications, entertainment, and health care. By the end of the decade, the combined equity market value of such firms was over \$15 billion, even though collectively they had reported no net income. These investments were based on the promise of the future. Value-creating business strategies were also pursued by older firms. In 1984, Northwest Industries was an investment-grade company that was purchased the following year through a leveraged buyout by Far-

ley Industries and subsequently downgraded. By 1987, its Fruit of the Loom subsidiary was taken public in a \$553 million stock and bond offering. Even though Fruit of the Loom increased its debt, the additional capital allowed the company to expand capital spending, create new jobs, and experience an increase of \$2 million in sales.

In 1988, Duracell was the battery division of Kraft Inc., and a AAA-rated company. That year Duracell was sold to a group that included management and outside investors. Duracell found itself a leveraged, noninvestment-grade company which no longer enjoyed ready access to inexpensive capital through its former parent, Kraft. But Duracell's president noted that it wasn't debt that inhibited the company's growth; it was being owned by a cheese company. Through innovation in new products and marketing, Duracell was able to increase annual pretax earnings nearly sixfold, from \$39 million at the time of its buyout to \$226 million by the end of 1990.

During the 1980s, following traditional European models of handling businesses in distress, exchange offers and restructuring packages for companies staving off bankruptcy became common. In manufacturing and energy companies hit by the collapse of prices in the early 1980s, exchange offers allowed companies to swap high-coupon debt issues for lower-coupon debt issues or equity. Access to interest rate and currency swaps, futures, and options also afforded opportunities in debt management that did not exist in earlier periods. Regrettably, tax code and regulatory changes in the late 1980s began to restrict these financial innovations and reduced the capacity of companies to adapt to changing market conditions.

The benefits of corporate restructuring and capital access should have been obvious. No longer were corporate leaders able to devote unused borrowing power and large free cash flows to low-benefit or even value-destroying investments. Contrary to conventional wisdom, most of the barbarians in corporate America had been inside the gates partying while the country was losing international markets. The resulting wave of corporate restructuring witnessed in the 1980s was salutary. From 1978–1988, firms having a single business segment, and operating that segment with increasing profitability, rose from 35.6 percent to 54.3 percent of all firms. Moreover, as Gregg Jarrell of the University of Rochester

has shown, the trend toward better focus was associated with significantly greater returns to shareholders, improved market share, and better accounting measures of firm performance.⁴

The restructuring of the 1980s for the most part undid the unsound acquisitions of the previous decades, leading to a deconcentration of industries and a deconglomeration of unwieldy units in unrelated lines of business. These developments came to an abrupt halt in 1989 as political forces mobilized to slow the pace of economic change through an unprecedented wave of financial regulation and litigation. This may well have helped to hurl the country into a recession, end job growth, and set the competitive position of leading industries drifting toward economic entropy.⁵

Journalism or Scholarship?

Nevertheless, the new financial regulations and litigation were probably supported by most people because of the widespread unfavorable publicity devoted to mergers and acquisitions in the 1980s. On a scholarly level, Walter Adams and James W. Brock, who had previously underscored the problems of bigness in business organization, have contributed to the demonization of business reorganization during the 1980s in their *Dangerous Pursuits: Mergers and Acquisitions in the Age of Wall Street* (New York: Pantheon, 1989).

Brock and Adams's wisdom about the 1980s is familiar at best. The first section of the book develops the metaphor of securities markets as casinos. Wall Street activities are chided as a "paper chase" pursued by business "arrivistes." The evidence for this view of financial markets consists largely of business press accounts of the decade's 20 largest deals (tables 2, 4, 5, 14, 40, and 41). New owners of corporate assets are, of course, labeled "raiders" (31-56), and transactions are analyzed in terms of the distribution of their proceeds (42) rather than in terms of resulting corporate performance. The fully disclosed financial advisory fees paid to financial intermediaries ("marriage brokers") (75-77) are deemed unseemly and excessive, apparently because they were often simply large, while the corporate and economic benefits made possible by the restructuring performed by these intermediaries are ignored.

Indeed, the authors suggest that the macro- and microeconomic

impact of mergers and acquisitions was almost completely negative, basing this conclusion on newspaper accounts of failed transactions in the 1980s (90–93) and studies of mergers and acquisitions during earlier waves of business combinations in the 1960s and 1970s (84–87). No systematic evidence of the results of merger and acquisition activity in the 1980s, either in the aggregate or by sector, is presented. Other effects are implied by conflating trends in merger and acquisition activity, research and development spending, and net new nonresidential investment (122–23). The authors interpret these trends, without the benefit of any econometric or multivariate analysis to control for other economic, tax, interest rate, trade, or organizational factors, to imply that increases in mergers and acquisitions caused research and development stagnation and declines in investment. The book concludes with a typology of “cow-boy” (1980s), “coalition” (corporatist), and “creative” (industrial, e.g., Ford/GM) capitalism (143–71) and a call for more stringent financial and merger regulations and punitive taxes on capital transactions, business combinations, and securities markets.

Still fighting the antitrust wars that characterized previous periods of mergers and acquisitions, Adams and Brock fail to consider a large body of empirically oriented literature that leads to much more balanced views of what happened to corporate structure in the 1980s.⁶ This literature suggests that the restrictions imposed on capital access and ownership change in the late 1980s helped to prop up established corporations that chose not to respond to challenges posed by upstart competitors and active investors to their market share and their stranglehold on capital. Established corporate interests, however, enjoyed only a temporary reprieve. Unchallenged by takeovers, the entrenched managers of such corporations as Sears, IBM, General Motors, Westinghouse, and American Express—all of whom were major supporters of antitakeover measures—led their companies toward massive losses in market share and employment. In the early 1990s, institutional investors have returned to the agenda of change first pursued by financial entrepreneurs and competitors in the 1980s, shifting lethargic corporations away from failed strategies.

Technological and economic changes accompanying the globalization of markets have made speed the watchword of business management. Ownership, capital structure, product strategies, and marketing plans must swiftly adjust to changing circumstances if

jobs, asset values, and whole corporations are not to disappear into the vortex of economic change. Corporate change is a "dangerous pursuit" only if it is resisted, thereby amplifying the damage of pursuing negative present-value investment strategies that ultimately destroy jobs and value within the firm, such as the ill-fated robotization program adopted by General Motors in the 1970s and the failed exploration strategies of big oil companies even as oil prices fell after 1974.

The mainstream reaction to ownership change and capital access in the 1980s, well articulated in *Dangerous Pursuits*, amounted to an attempt to reduce the pace of change, thereby prolonging unsuccessful corporate strategies and increasing their future costs. In failing to differentiate mergers and acquisitions that *adapted* to existing corporate and industrial organization structures (those pursued by the largest corporations to entrench their corporate power) from those that tried to *change* corporate and industrial structures (those pursued by financial entrepreneurs, management, employee owners, and active investors), Adams and Brock sidestep arguments and evidence that present a more differentiated view of the 1980s.

Few complaints about excess debt or takeovers were heard prior to the mid-1980s. In fact, the Business Roundtable and Fortune 500 companies that suddenly promoted new regulations on credit access and ownership change in the mid-1980s had opposed restrictions on takeovers during the Carter years, when they were the only companies able to finance large-scale acquisitions. But when merger and acquisition departments on Wall Street discovered high-yield financing as a means to launch takeover bids, old-money firms and their white-shoe investment banks sought protection against takeovers not by going to their shareholders, who might be attracted to the financial opportunities offered by the challengers, but to judicial, legislative, and regulatory bodies that could be lobbied on the basis of political contributions instead of economic returns.

While takeover battles set loose extraordinary animosity toward ownership change and debt, the market for corporate control was not the only area where entrenched and protected managers were losing ground. Market share in major industries was being eroded by entrepreneurial newcomers (such as MCI, the Turner Broadcasting System, and MTV) that deftly leveraged growth through

what was then the lowest-cost type of capital—debt. Whether the assault on corporate power appeared as a takeover bid or through the appearance of a new, more technologically and financially adept market competitor, however, the common cause of concern for the corporate status quo was the access to capital that had been created by new securities and by nonbank financial institutions. This access allowed markets, not institutionally concentrated financial power, to determine the cost and allocation of capital.⁷

Takeovers, Makeovers, and the Market for Corporate Control

This century's fourth merger wave began in 1981 and was the longest and perhaps most controversial of all. Unlike earlier eras of economic and ownership restructuring, the latest one occurred during a period when the relative position of U.S. firms in the world economy was declining. Heightened global competition, deregulation, and technological and demographic changes forced corporations to enhance efficiencies, streamline their operations and develop cost-effective strategies.⁸

In part, these challenges were the vestiges of earlier waves of merger and acquisition activity that had resulted in greater industrial concentration. Adams and Brock cite their own work and that of Ravenscraft and Scherer and others demonstrating the negative effects of the tendency toward conglomeration by acquisition that occurred in the 1960s and 1970s (84–87). Regrettably, most of their data stop in 1981, when deconcentration and deconglomeration began to threaten the dominant merger and acquisition strategy of the largest firms. At this point small businesses replaced industrial giants as the motor of economic growth.⁹ Moreover, while small firms had increasingly served as suppliers and distributors for the largest firms as the seemingly ineluctable pattern of industrial concentration advanced,¹⁰ new technologies and markets to which smaller firms could respond more flexibly now began to break the dependency of small firms on large ones. Small and medium-sized firms began to dominate emergent industries and markets that had shorter product life cycles.¹¹ Thus, while the earlier waves of mergers and acquisitions—at the turn of the century, in the 1920s, and in the late 1960s and early 1970s—led to increased levels of economic

and industrial concentration, this tendency was reversed in the 1980s.

Adams and Brock are by no means alone in failing to differentiate between defensive and conglomerating mergers and acquisitions on the one hand, and on the other, the more productive mergers and acquisitions of the 1980s. This failure deflects attention from failed public and business policies pursued by political and management interests that sought to inhibit change.¹² Admittedly, even in the 1980s, traditional takeovers serving conservative purposes ultimately outnumbered the innovative takeovers of medium- and large-sized companies by financial entrepreneurs, management, and employee owners who used new forms of debt and equity financing (junk bonds, securitization, etc.). From 1980 to 1988, according to the Securities and Exchange Commission, high-yield debt financed 10 percent of tender offers while banks provided 73 percent of tender-offer financing. In hostile takeovers, bank borrowing accounted for 78 percent of the financing. Of the \$215 billion raised in the high-yield market during the 1980s, only 23 percent was used to finance leveraged buyouts and repayment of leveraged buyout debt.¹³ Most takeovers continued to be made by the largest companies, often without debt financing.

The difference in the 1980s was that often the largest firms now faced smaller competitors in these acquisition bids.¹⁴ Leveraged buyouts never exceeded 23 percent of the total value of merger and acquisition activity. Big companies made the majority of offers for other big and small companies. By the early 1980s, law firms and investment banks saw new potential deal flow in takeovers.¹⁵ Of the top 100 transactions since 1900 (hostile and friendly), only 14 percent used high-yield financing. Eighty-six percent of those transactions were made by large domestic or foreign corporations. The first recorded hostile takeover was made in 1974 by International Nickel Co. of Canada with the financial advice of Morgan Stanley & Co., using no debt financing.

Adams and Brock are thus more than half right in arguing that the bulk of merger and acquisition activity has failed to produce the hoped-for benefits for shareholders of the acquiring company, employees, or communities. This is largely true of the big, "strategic" acquisitions by big businesses. But it is not true of smaller, entrepreneurial acquisitions, as demonstrated in the emerging body of economic research about corporate restructuring.

It is easy to see why political debate failed to make this distinction. Politicians and mass opinion naturally tend to assume that the large payrolls of big companies (especially those with established political ties and records of supporting local, state, and federal legislators) serve the public interest. The public interest in corporate efficiency is much more abstract. Scholarly analysts should be more discerning, but this cannot happen when they lump together all mergers and acquisitions, however disparate their sources and outcomes.

Most “strategic” acquisitions by large corporations were ill-conceived forays into unfamiliar lines of business that eventually resulted in painful write-offs and divestitures of assets, accompanied by layoffs in the acquired companies. Of the \$68.7 billion of corporate assets acquired in hostile takeovers between 1984 and 1986, the peak period of hostile activity, \$49.7 billion, or 72 percent, eventually fell into the hands of “strategic” buyers—corporations that were expanding existing lines of business through acquisition or were moving into new fields. These strategic buyers, however, kept only 52 percent of the assets they acquired, roughly \$26 billion worth, and in turn sold off the remaining assets, \$23.7 billion worth, to other strategic buyers.¹⁶

Debt-Bashing: Where Credit Is Due

Federal, consumer, and corporate debt each grew in the 1980s, but with different macroeconomic outcomes. How can we judge whether, as Adams and Brock argue, corporate debt was excessive?¹⁷

Three major sources of data are available to assess the macroeconomic characteristics of corporate debt: the ratio of debt to the domestic output of nonfinancial corporations; net interest as a share of gross cash flow (i.e., the amount of net cash generated by a business); and the ratio of short-to long-term debt.

From the early 1950s through the mid-1970s, biases in the tax code, which permitted the deduction of interest income, led corporations to steadily increase their dependence on debt. Each business cycle saw greater levels of corporate indebtedness. The 1980s were no different. But in that decade, while corporate debt ratios eventually grew, they remained well below the levels of the mid-1970s.

Net interest as a share of gross cash flow never reached a level that was substantially higher than it was in the 1970s. Short-to long-term debt ratios fell as firms sought more predictable financing and more efficient sources of low-cost capital.

Relative to the underlying market value of the equity growth achieved by debt, the corporations of the 1980s do not appear overly leveraged. Debt/equity ratios actually showed a decline from 84 percent to 74 percent. The debt content of corporate GNP was actually below trend, and the debt content of corporate obligations to net cash flow was consistent with trends over the previous 40 years. The capacity to service debt (income divided by interest payments) remained relatively stable even though absolute levels of debt rose.

The 1980s were, therefore, more a decade of equity than a decade of debt. Equity growth exceeded debt growth. During the 1980s, the value of the stock market tripled to \$3 trillion. As one observer put it, "at the end of 1992 the median *Forbes* company had more than \$2 in equity capital for every dollar in debt; 69 cents in equity out of each capital dollar. . . . If you compare debt to market value rather than to book value, the equity ratio rises higher than 2 for 1."¹⁸

Acquisitions did increase debt. By the late 1980s, many acquisitions were overpriced, but even these did not cause the aggregate disruptions in the economy that have occurred since the growth decade ended. Many of these acquisitions were strategically motivated and fostered additional growth and gains in productivity. As John Paulus points out,

despite the debt incurred to finance takeovers and restructurings, for every dollar of increased indebtedness in the 1980s, non-financial corporations acquired 80 cents of financial assets plus \$1.52 of tangible assets. Thus, while debt expanded swiftly, non-financial corporations added a total of \$2 in income earning tangible and financing assets for every dollar of increased credit market debt in the 1980s. This hardly seems to indicate that corporations are drowning in a sea of red ink, or that they lack sufficient resources to service their debt.¹⁹

Relative to major foreign competitors, U.S. companies remain underleveraged. In the manufacturing sector during the 1980s, German and Japanese corporations averaged 69.5 percent and 55.7 percent of debt in their respective capital structures, compared with 45.3 percent for similar U.S. firms. Are higher levels of debt neces-

sarily harmful? Compared with U.S. manufacturers, foreign manufacturers showed a lower after-tax cost of capital, higher volume, and higher profitability growth rates in the 1980s. Yet as a proportion of gross economic output, U.S. corporate debt lags considerably behind Germany, Japan, and Canada.²⁰

Build vs. Buy: Ownership Change and Growth

Adams and Brock never tire of the primitive notion that corporations were put on this earth to build things, not buy them (165-67). In this view, acquisitions are parasitic on physical processes of value creation. The labor theory of value dies hard.

Economic analysis suggests a less moralistic posture toward acquisitions, although as I have pointed out, not all acquisitions serve productive purposes. As market and business conditions change, so too should the ways in which a corporation invests and finances its activities.

The origins of credit access, expansion, and ownership change in the 1980s goes back to the credit crunch of 1974, when companies learned that they could not take bank financing for granted. New service, communication, health, and science industries needed money to grow. Older industries, such as automobiles, farm equipment, mining, and steel, needed huge sums to rebuild in order to survive and compete in a changing economy.

In the mid- to late 1970s, as interest rates were stabilizing below 10 percent and equity market values were falling relative to replacement values, many corporations used debt-oriented securities to finance their expansion and growth. Because of high replacement values, it often made sense to acquire existing assets and add value to them through technology and marketing, rather than build them from scratch.²¹ These conditions persisted until the late 1980s, when market values rose above replacement costs. Market conditions then called for equity-oriented financing and investments in new plants and equipment.

In the 1980s, productive mergers and acquisitions resulted in restructuring and deconglomerating corporations. Divisional divestitures or takeovers often resulted in organizational efficiencies that yielded higher rates of marginal revenue.²² Vertical and horizontal integrations, product line extensions, and market extensions played

important roles in acquisition strategies. Some acquisitions consolidated excess capacity in various industries and enabled a number of low-margin, low-market-share operations to join forces as higher-margin, higher-market-share businesses (e.g., Stone Container in box manufacturing, Medco Containment in pharmaceuticals, Duracell in batteries).²³

A second problem with Adams and Brock's attack on corporate acquisitions in principle is that "build" and "buy" strategies are not mutually exclusive. In the inflationary mid-1970s, money managers in insurance companies and mutual funds were able to make investment decisions without the restrictions that go with the government subsidies and guarantees that are retained by banks and savings and loans. The result was the emergence of the noninvestment-grade debt market. Yet of the \$215 billion of high-yield debt raised in the 1980s, the lion's share (77 percent) was used for expanding or rebuilding firms, not for acquisitions. Some of the most innovative and creative industries of the last decade, including home building, low-cost pharmaceuticals, health care, cable television, long-distance services, and cellular communications, turned to this portion of the financial market. They built as they bought, and responded to social needs in the process.

The perception of excessive debt comes from confounding historical cost accounting measures that are snapshots of the past with market values in a dynamically growing marketplace. Traditional accounting methods measure cost, not value—yesterday, not tomorrow.

Conventional accounting standards that focus only on book values, original costs, and historical performance fail to reflect the potential value of an enterprise. According to Federal Reserve data, the market value of nonfinancial U.S. companies as of January 1, 1970, exceeded their debt by more than \$300 billion. During the next five years, values would decline so that by 1974, debt actually exceeded equity by nearly \$20 billion, even though corporations weren't necessarily borrowing more. The recovery of stocks, which began in 1975, stimulated equity values to rise \$180 billion relative to debt over the balance of the 1970s. Still, by the end of the decade, equity exceeded debt by only \$160 billion, well below the level of the previous decade. The recovery of equity securities in the latter half of the 1970s laid the groundwork for the tripling of stock market values in the 1980s. This dramatic turnaround resulted

in equity exceeding debt by over \$1.2 trillion in nonfinancial companies, a nearly eightfold increase from \$160 billion in 1979.

Mergers, Acquisitions, and Wealth Redistribution

Adams and Brock (90–106) echo conventional wisdom in claiming that whatever the efficiency gains they produced, business combinations, takeovers, mergers, and acquisitions upwardly redistributed wealth and income from labor, the government, and consumers.²⁴

The empirical evidence regarding job growth and loss is unambiguous. Perhaps surprisingly, the largest firms, which had a stranglehold on capital access and mergers and acquisitions for most of this century, were the ones that eliminated jobs in the 1980s. The 800 investment-grade firms decreased employment by 4 percent over the 1980s, while noninvestment-grade companies experienced a 24 percent increase in employment.²⁵ Among Fortune 500 companies during the same period, job loss was around two million while the economy as a whole added 12 million jobs.

Businesses have two ways to sustain or increase profits—increase revenues or decrease costs. In the 1980s, smaller companies in growing and emergent industries (e.g., cable, health care, restructured manufacturing, telecommunications) generally utilized the former strategy, while the largest established companies chose the latter. Throughout the decade, the largest companies sought higher profits by cutting costs rather than by generating new revenues through research and development, new products, and new markets. Cutting costs, of course, often means cutting the payroll. Only 4.4 percent of the mass layoffs in the 1980s, representing 6.6 percent of the total jobs lost in shutdowns, resulted from ownership change. Of 1100 plants involved in 110 LBOs, plants involved in management and employee buyouts were substantially less likely to close than other plants.²⁶

Moreover, high-yield and restructured companies in sectors like manufacturing, which were hard hit by import pressures and declining demand, showed higher rates of job retention than the rest of the firms in their industries. Manufacturing companies like Mattel, Seminole Kraft, and National Can actually restored jobs in plants previously threatened by closings. In plants that were either

divested or restructured through buyouts, productivity growth was about 14 percent higher than that of other plants in the same industries. In plants with significant management participation, productivity growth was 20 percent higher. Research and development expenditures were also higher than in comparable industry facilities.²⁷

This is not to say that there were not very poorly structured transactions that resulted in job losses. This was particularly true in those defensive LBOs and corporate acquisitions in which entrenched managers took on more debt to defend against takeovers (e.g., Freuhauf). Two-thirds of the "distressed credits" in the 1980s were issued in the last few years of the decade, when deal-hungry merger and acquisition departments overpriced issues in poorly structured, fee-driven transactions.²⁸ Entrepreneurial buyouts that added strategic economic value were superseded in the LBO market by defensive financially driven buyouts. In these "friendly deals," old management was consolidated, resulting in companies that were bought at too high a price, run by failed managers who maintained their empires with undercapitalized, overleveraged financial structures. The market responded, however: deal and bond prices fell in late 1989 as investors learned that all LBOs were not created equal. This self-correcting process came to an end with Treasury Secretary Brady's imposition of regulations that effectively ended all innovative buyouts.

Layoffs occur infrequently after takeovers. The major sources of job loss that can be associated with takeovers are those that affect high-level, white-collar employees. Middle-management positions, especially administrative staff, are often cut as overhead is reduced, but production plant workers are largely unaffected.²⁹ After pruning, employment usually grows. Firms experiencing ownership changes have higher employment and wage levels, as well as greater productivity increases, than firms that do not change control.³⁰ Layoffs have turned out to be higher when entrenched managers defeat a takeover bid than when takeovers are successful.³¹ In a sample of 286 plant closings, only 48 (or 17 percent) were made by takeover targets either before, after, or during the bid—and only 22 of these 48 were targets of hostile bids.³²

In short, job losses do not appear to be attributable to ownership change. This is not to say that labor's benefits are maximized by ownership change—unless workers become owners, as in the case

of employee ownership buyouts. In fact, many of the financial strategies for leveraged buyouts were initially developed for employee stock ownership plans. The best prospect for maximizing labor benefits is not to stop ownership change (as most labor unions have attempted to do), but to be at the table as employee owners (e.g., Avis, Weirton Steel, and Oregon Steel).³³

Adams and Brock contend (83-84) that takeovers transfer wealth from the public budget through tax expenditures that encourage debt and acquisition, such as interest deductions for debt, accelerated depreciation deductions from a step-up in asset basis upon purchase, and the creation of favorable tax attributes, such as net operating losses. But a comprehensive study of several hundred mergers over the period 1968-1983 found that only 20 percent generated tax benefits, and that the estimated value of these benefits was much lower than were the takeover premiums, averaging 14 percent of the acquired firm's market value.³⁴ More importantly, Brock and Adams neglect to take account of the tax revenue generated by the positive consequences of takeovers. The net impact of LBOs on the U.S. Treasury has been positive, not negative, when one includes the increased tax payments of the new debt holders and those shareholders who sold their stock in the targeted companies, as well as the firms' future higher income from productivity increases.³⁵

Federal antitrust laws are explicit in restricting a company's ability to obtain monopoly power from mergers and acquisitions that could negatively affect consumers through administered, noncompetitive pricing. The more refined studies of postacquisition corporate performance suggest that returns to acquirers in unrelated conglomerate acquisitions in the 1980s were lower than the returns to related acquisitions.³⁶ There is some evidence that these returns were not the result of increased market power, but of gains in economies of scale or in productivity after the acquisitions. In at least one study, cash flows of merged or acquired companies increased after the mergers, but sales margins did not.³⁷ Asset productivity, instead of monopoly pricing, improved cash flows, suggesting that consumers were no more hurt by mergers and acquisitions than were workers.

* * *

The only dangerous pursuits demonstrated in this book are those

of tabloid economics. The authors never succeed in going beyond the headlines to explore the complex dynamics of the market for corporate control, the processes of restructuring, or the history and structures of business change. All investment anticipates the future and assigns an expected return. By pejoratively describing all economic exchanges based on future returns as "speculative," the authors fail to enlighten us about how financial markets work, sometimes imperfectly, and why.

Surely analyses such as Adams and Brock's should be based neither on selective samples of the largest transactions in a few industries nor on anecdotes about greed and envy on Wall Street or Main Street. The emerging body of empirical economic research provides a much more variegated and systematic analysis of the strengths and weaknesses of corporate change.

Consistent with the dynamics of other complex markets, neither merger and acquisition activity nor the financial markets that enable it are inherently good or bad. Under certain historical conditions these financial technologies can boost efficiency, welfare, employment, and the satisfaction of consumer demand. Still, several general conclusions can be drawn:

1. Corporate focus through diversification, divestitures, and same-industry acquisitions improves all economic outcomes (employment, productivity, efficiency, and gains in market share).
2. Defensive restructurings are most likely to fail; capital structures with the lowest costs (through debt or equity) are most likely to succeed.
3. Whether management is by active investors, employees, or managers makes a big difference in performance incentives and outcomes.
4. Regulatory measures targeted at slowing change in ownership, technology, industrial structure, or strategy exacerbate negative outcomes.

Efficient markets require increased investor participation; the greater the participation, the greater both the technical efficiency and the social benefits (as measured by income and wealth generation). The short-cuts, cheating, and price distortion Brock and Adams assume are the rule rather than the exception tend to disappear when information in markets becomes less asymmetrical. Conversely, efforts to restrict competition and new market entrants so as to ensure stability end up producing the opposite result.

When the state intervenes to restrict capital access and market participation, and creates systematic biases against smaller and newer entrants by raising transaction costs (legal, financial, or regulatory), the outcome is lackluster economic performance of the sort we are now experiencing and the perpetuation of businesses that fail to keep economic growth alive.

NOTES

1. See David Birch, *Job Creation in America* (New York: Free Press, 1988), W. J. Greider, *Secrets of the Temple: How the Federal Reserve Runs the Country* (New York: Simon and Schuster, 1987), and Mark Gartler, "Financial Structure and Aggregate Economic Activity: An Overview," *Journal of Money, Credit and Banking* 20, no.3 (August 1988): 559-96.
2. Securitization refers to the array of creative financing techniques, many of which were used in structuring hybrid and debt high-yield instruments, which enable depository institutions, finance companies, and other corporations to "liquefy" their balance sheets (i.e., raise cash by borrowing against assets) and develop new sources of capital.
3. See G. Yago and J. Tannenbaum, *The Economic Impact of IPOs on U.S. Industrial Competitiveness* (Washington, D.C.: National Association of Securities Dealers, 1989).
4. Robert Comment and Gregg A. Jarrell, "Corporate Focus and Stock Returns," William E. Simon Graduate School of Business Administration, University of Rochester, May 1991.
5. Regulatory constraints exacerbated the 1990-1991 recession by choking off investment capital at a critical point in the recession and prolonging the shallowness of the recovery; see David Bizer, "Regulatory Discretion and the Credit Crunch," working paper, U.S. Securities and Exchange Commission, 1993; Michael Jensen, "Corporate Control and the Politics of Finance," *Journal of Applied Corporate Finance* 4, no. 2 (1991): 75-109; Ben Bernanke and C. S. Lown, "The Credit Crunch," *Brookings Papers on Economic Activity* 2 (1991): 205-47; and G. Yago, "Financial Repression and the Capital Crunch Recession: Political and Regulatory Barriers to Growth Economics," in B. Zycher and L. C. Solmon, eds., *Economic Policy, Financial Markets and Economic Growth* (Boulder: Westview Press, 1993), 81-114. By contrast, all macroeconomic explanations of the recent recession are unsupported. See Robert E. Hall, "Macrotheory and the Recession of 1990-91," working paper, National Bureau of Economic Research and Stanford University Department of Economics, January 1993. All business cycle variables, monetary factors, and other major theoretical models of the economy fail to explain the downturn and leave as a residual in the resulting econometric models an unexplained drop in consumption.
6. See M. Jensen, "Agency Costs of Free Cash Flow, Corporate Finance, and

- Takeovers," *American Economic Review* 76, no. 2 (May 1986): 384-425; Steven N. Kaplan, "The Staying Power of Leveraged Buyouts," *Journal of Financial Economics* 29 (1991): 287-313; Sanjay Bhagata, Andrei Shliefer, and Robert Vishny, "Hostile Takeovers in the 1980s: The Return to Corporate Specialization," *Brookings Papers on Economic Activity: Microeconomics* (1990): 1-72; John Easterwood and Anju Seth, "Strategic Restructuring in Large Management Buyouts," *Strategic Management Journal* 13, no. 2 (May 1993): 43-62; and Comment and Jarrell, n4 above.
7. Bank loans and noninvestment-grade corporate debt are close substitutes. Upon the demise of Drexel, Burnham, Lambert, large financial institutions were able to resume their resistance to market innovations and experience a wealth transfer through the return to traditional intermediated funding. See L. M. Benveniste et al., "The Failure of Drexel, Burnham, Lambert: Evidence on the Implications for Commercial Banks," *Journal of Financial Information* 3, no. 1 (March 1993): 93-138.
 8. For an accessible discussion of the competitiveness issue, see G. N. Hatso-poulos, P. R. Krugman, and L. Summers, "U.S. Competitiveness: Beyond the Trade Deficit," *Science* 15 (July 1988): 299-307.
 9. See D. J. Storey and S. Johnson, *Job Generation and Labor Market Change* (New York: Macmillan, 1987), and David Birch, Anne Hagerty, and William Parsons, *Who's Creating Jobs?* (Boston: Cognetics, 1993).
 10. See John Paulus and Robert Gay, "Is America Helping Herself? Corporate Restructuring and Global Competitiveness," *World Economic Outlook* (New York: Morgan Stanley, 1987).
 11. This has been true in steel (e.g., U.S. Steel versus Nucor), communications (ATT versus MCI), broadcasting (the networks versus the cable industry), retail (Sears versus WalMart), and computing (IBM versus Microsoft), and is consistent with research and development within corporations. See F. Lichtenberg, "The Private R & D Investment Response to Federal Design and Technical Competitions," *American Economic Review* 78, no. 2 (June 1988): 550-59.
 12. Buyout capital structures by the late 1980s were used to preserve existing ownership threatened by takeovers. Most of these failed, and comprised the greatest number of the "bad cohort" of transactions of the 1987-1988 period, which eventually accounted for two-thirds of the distressed companies in the high-yield market by 1990. See Steven Kaplan and Jeremy Stein, "The Evolution of Buyout Pricing and Financial Structure in the 1980s," *Quarterly Journal of Economics* 23, no. 2 (Spring 1993): 88-119, and Mark Mitchell and Ken Lehn, "Do Bad Bidders Become Good Targets?" *Journal of Applied Corporate Finance* 3, no. 2 (Summer 1990): 60-69.
 13. See Edward Altman, "Setting the Record Straight on Junk Bonds," *Journal of Applied Corporate Finance* 3, no. 2 (Summer 1990): 82-95, and General Accounting Office and Securities Exchange Commission, *Financial Markets: Issuers, Purchases and Purposes of High Yield Investment Grade Bonds* (Washington, D.C.: G.P.O., 1988).

14. See G. Yago, *Junk Bonds* (New York: Oxford University Press, 1991), 108–60.
15. Ironically, many of the most visible debt bashers came from these firms. Martin Lipton (of Wachtell, Lipton), who coined the phrase “two-tiered tender offer, junk bond, bust up, shutdown takeover,” built a profitable retainer practice in the takeover market by offering target firms the protection of his firm by paying a preemptive retainer that would ensure that his services would not be used by corporate raiders. Similarly, Felix Rohatyn (who referred to high-yield securities as “securities swill”) and his firm, Lazard, Freres, built an important practice defending against takeovers.
16. Bhagata, Shliefer, and Vishny, n6 above. The results of these large corporate strategies were underwhelming and forgettable: e.g., the Dow Jones acquisition of Telerate; the General Electric acquisition of Kidder, Peabody; the General Motors acquisitions of Hughes and EDS; Mobil’s acquisition of Montgomery Ward; Kodak’s acquisition of Sterling Drug; and Westinghouse, Control Data, Union Carbide, Texaco, IBM, and others that, though no longer “threatened” by raiders or junk bonds after the virtual government regulatory shutdown of the high-yield market and the market for corporate control, performed miserably in the 1990s and have slashed jobs and asset values.
17. See Adams and Brock’s debt-bashing arguments on pp. 175–81. Blithely they cite Theodore Forstmann’s characterization of junk bonds as “the fake wampum of 1980s finance,” apparently unaware that Forstmann’s LBO firm, Forstmann-Little, financed its buyouts with *privately* raised subordinated debt from pension funds and insurance companies and was therefore in constant competition with high-yield *publicly* generated subordinated debt securities that supported competitive bids.
18. James Michaels, “All Our Aching Angst,” *Forbes*, September 14, 1992: 43.
19. John Paulus, *World Economic Outlook* (New York: Morgan Stanley, 1992).
20. See George Hatsopoulos, *High Costs of Capital* (Washington, D.C.: American Business Conference, 1987).
21. See Lorraine Spurge, *An American Renaissance: The Democratization of Capital* (Los Angeles: Knowledge Exchange, 1991), and idem, *The High-Yield 500* (Los Angeles: Knowledge Exchange, 1992). In short, it becomes cheaper to buy a company and rebuild it than to build one from scratch.
22. See Tim C. Opler, “Operating Performance in Leveraged Buyouts: Evidence from 1985–89,” *Financial Management* 12, no. 2 (Spring 1992): 52–75. Larger increases in operating cash flow resulted from these activities.
23. Recent developments confirm the innovativeness and value of these strategies. In the recent boomlet of merger and acquisition activity in 1993, many transactions represent the attempt of larger companies to absorb the dynamism and markets of their more adept, high-yield-financed competitors. For example, British Telecom bought 20 percent of MCI, whose market capitalization rose from \$4.5 billion to \$20 billion in the past five

years. Similarly, the pharmaceutical giant Merck acquired Medco Containment, and Bell Atlantic acquired TCI.

24. See Roberta Romano, "A Guide to Takeovers: Theory, Evidence, and Regulation," *Yale Journal on Regulation* 9, no. 1 (1992): 22-65.
25. See Yago, *Junk Bonds*, chs. 6-8.
26. *Ibid.*, 127-32.
27. Glenn Yago, Frank Lichtenberg, and Donald Siegel, "Leveraged Buyouts and Industrial Competitiveness: The Effects of LBOs on Productivity, Employment, Research and Development," working paper no. HAR-90-003, W. Averell Harriman School of Management and Policy, State University of New York at Stony Brook, 1989.
28. See Martin Fridson, "High Yield Year in Review," *Extra Credit* 7 (January 1991): 3-10.
29. Frank R. Lichtenberg and Don Siegel, "Productivity and Changes in Ownership of Manufacturing Plants," in Martin N. Bailey and Clifford Watson, eds., *Brookings Papers on Economic Activity* (Washington, D.C.: Brookings Institution, 1987).
30. *Ibid.*; Charles Brown and James Medoff, *The Impact of Firm Acquisitions on Labor* (Washington, D.C.: National Bureau of Economic Research, 1987).
31. See Yago, *Junk Bonds*, 151-60, and Kaplan, 300-313.
32. David W. Blackwell, Wayne M. Marr, and Michael F. Spivey, "Plant-Closing Decisions and the Market Value of the Firm," *Journal of Financial Economics* 26 (1990): 136-58.
33. The greatest theoretical and practical contribution on the issue of employee ownership and more broadly, binary economics, is to be found in the work of Louis Kelso. See particularly Louis O. Kelso and Patricia H. Kelso, *Democracy and Economic Power: Extending the ESOP Revolution* (Cambridge, Mass: Ballinger, 1986). An excellent exposition and development of Kelso's theoretical model is to be found in Robert H. A. Ashford, "The Binary Economics of Louis Kelso: The Promise of Universal Capitalism," *Rutgers Law Journal* 22, no. 1 (Fall 1990): 3-121. See also Daryl D'Art, *Economic Democracy and Financial Participation* (New York: Routledge, 1992).
34. Alan J. Auerbach and David Reishus, "The Impact of Taxation on Mergers and Acquisitions," in Alan Auerbach, ed., *Mergers and Acquisitions* (Chicago: University of Chicago Press, 1988).
35. Michael Jensen, Steven Kaplan, and Laura Stiglin, "The Effects of LBO's on Tax Revenues," *Tax Notes* 42 (1989): 22-54.
36. Paul Healy, Krishna Palepu, and Richard Ruback, "Does Corporate Performance Improve After Mergers?" *Journal of Financial Economics* 31 (1992): 242-64.
37. Jonathan M. Karpoff and Paul H. Malatesta, "The Wealth Effects of Second Generation Takeover Legislation," *Journal of Financial Economics* 25 (1989): 19-43.

JUNKING CORPORATE AMERICA

A firestorm of deals and debt consumed corporate America during the 1980s. Some contend that this deal mania, and the junk debt that fueled it, enhanced the nation's economic performance and bolstered its global competitiveness. Viewed in the context of its aftermath, however, the evidence suggests that a decade of junk-debt deals subverted economic performance, weakened the country's economy and rendered it dangerously vulnerable to recession, and inflicted a massive opportunity cost vis-à-vis America's foreign rivals.

The story is told of the wonder-rabbi of Chelm, who saw in a vision the destruction by fire of the study house in Lublin, 50 miles away. Weeks later, visitors arriving in Chelm from Lublin were met by the wonder-rabbi's disciples with expressions of sympathy and concern, tinged with pride in their master's prophetic prowess. "What on earth are you talking about?" the travelers asked. "We left Lublin just yesterday. The study house is standing as sturdy as ever. Just what kind of wonder-rabbi is this that you have?" "Well," the rabbi's disciples replied, "burned or not burned, it's only a detail. The wonder is that he could see so far."

Glenn Yago also has a vision. In *Junk Bonds: How High Yield Securities Restructured Corporate America* (New York: Oxford Univer-

sity Press, 1991), he examines the junk-bond binge that consumed corporate America during the 1980s, and pronounces it good. His studies, supported in part by Drexel Burnham Lambert¹ (predominant purveyor of junk debt until its 1990 bankruptcy), and acknowledging an "enormous intellectual debt to Michael R. Milken" (king of junk and impresario of Drexel Burnham until his 1990 conviction of felonious violations of U.S. securities laws), Yago proclaims junk bonds "one of the most important breakthroughs in the effort by U.S. industry to restructure itself in the face of global competition" (v, x).

Based on his statistical regression analyses and case studies, Yago concludes that junk bonds and leveraged buyouts unleashed efficiency, productivity, profit, employment, sales, capital spending, and research and development (8-9, 177). They "provided a flexible, cost-effective way for companies to purchase assets, expand product lines, and initiate new projects. They also helped fund acquisitions that can increase a company's size and profit potential while improving its competitive position" (48). They provided the "significant leverage" which "creates a powerful incentive to increase efficiency and profitability," thereby reducing "the risk of business failure or default" (125). They "have been a valuable tool, helping corporations meet the challenges of an increasingly competitive world" (196). Junk-debt deals benefited everyone—managers, workers, shareholders, investors, firms, the economy, and the nation (223).

Alas, like the rabbi's prophecy, this vision is refuted by the facts.

An Eruption of Deals and Debt

The 1980s were the decade of the debt-laden, junk-powered corporate deal. Mergers, takeovers, and leveraged buyouts erupted in number, from 1,565 in 1980 to 4,323 by 1986, with a cumulative total of some 26,671 deals conducted over the 1980-1988 period. The annual value of corporate deals skyrocketed from an estimated \$33 billion in 1980, to \$227 billion by 1988; the number of mega-deals valued at \$1 billion or more rose from 3 in 1980 to 42 in 1988. The cumulative value of corporate deals consummated during the decade exceeded \$1 trillion.²

Debt fueled this razzle-dazzle deal mania, and junk bonds pro-

vided an explosive additive: annual issues of corporate debt escalated 400 percent between 1980 and 1986 (from \$42 billion to \$212 billion);³ total outstanding corporate debt more than doubled over the decade, from \$829 billion in 1980 to \$2.1 trillion by 1990.⁴ For 10 years, American firms issued debt at the rate of \$1 million every four minutes, 24 hours a day.⁵ Junk debt jumped even more dramatically: annual issues of junk bonds spurted 2,150 percent between 1980 and 1986 (from \$2 billion to more than \$45 billion); total junk debt outstanding leaped 580 percent over the decade (from \$30 billion in 1980 to \$204 billion by 1989).⁶ By 1988, the financial structure of the typical leveraged buyout featured 87 percent debt, and a mere 13 percent equity—capital structures indicative of insolvency according to objective financial standards.⁷ Interest payments required to service these debt loads matched their frantic growth: more than half of corporate earnings were consumed by interest payments during the 1980s, compared with just 16 percent over the postwar period prior to 1970.⁸

Regressive Statistics

Yago presents statistical regression analyses which, he contends, scientifically compel his conclusions concerning the beneficence of this debt-financed deal mania. His statistics are unpersuasive.

First, he fails to distinguish growth caused merely by acquisitions of operations, on the one hand, from genuine growth due to internal expansion and actual managerial success, on the other (226). Deal makers who acquired one firm, then acquired another firm, and then another will, by Yago's algorithm, evidence great economic success when, in fact, all they achieved was a reshuffling of ownership of existing assets. No new plants, sales, profits, or employment need have been generated.

In addition, Yago limits his statistical analysis to firms that assumed junk debt and survived, but appears to exclude from his analysis junk issuers that failed (170). Not surprisingly, he finds that the successes succeeded.

Yago also fails to consider the accounting manipulations employed by deal makers to artificially create a façade of success.⁹ For example, interest payments on corporate debt can be deducted for tax purposes, while dividends paid to stockholders are not tax

deductible. Hence, leveraging by substituting debt for stock equity can substantially raise corporate after-tax cash flows, even though a firm's real economic efficiency is unchanged. Similarly, inflating the book value of an acquired firm's assets, and taking larger depreciation allowances to reduce taxes, creates an illusory growth in capital assets. Tax loss carryforwards from previously acquired operations also have been exploited to produce the appearance of enhanced economic performance, as has the raiding of pension funds held by firms following takeover.

Most conspicuously, however, Yago terminates his statistical analyses prior to the spectacular collapse of this frenetic deal mania.

The Conflagration of Bankruptcy

An all-time record-breaking rate of bankruptcies and defaults brought the decade of the leveraged corporate deal to a crashing close. Unregressed statistics tell the story: the annual number of corporate bankruptcies skyrocketed 538 percent between 1986 (the year when Yago's statistical analyses end) and 1990. The annual value of corporate assets entering bankruptcy soared 540 percent, from \$13 billion to \$83 billion; assets of firms filing for bankruptcy in 1990 were 50 times greater than 10 years earlier.¹⁰

Default rates on corporate debt escalated in similar fashion, from \$5 billion in 1988 to triple that amount by 1990.¹¹ Junk bonds, in particular, lived down to their name: the value of junk-debt defaults soared from \$12 billion in 1989 to \$25 billion in 1990; the rate of defaults on junk reached levels possibly as high as 24 to 38 percent.¹²

Entire industries were massacred by junk. By the end of 1990, one-fifth of all retailing industry junk debt was in default, a quarter of all airline junk was in default, and nearly half of all junk bonds issued by casino operations was in default.¹³ Failure rates for leveraged buyouts proved equally damning. According to one study, 25 percent of LBOs conducted in 1985 and 1987 defaulted on their debts by 1991; the default rate for LBOs conducted in 1986 reached an impressive 40 percent.¹⁴

The collapse of corporate creditworthiness was also reflected by the drastic rise in the ratio of downgrades to upgrades of corporate bond quality (as determined by independent bond-rating

agencies)—from approximately one to one during the 1970s, to two to one during the 1980s, and reaching an astronomical five-to-one level in 1990.¹⁵

A partial list of companies bankrupted by debt, as well as firms suffering serious financial problems due to excessive debt levels, includes: Campeau Corporation (including its Bloomingdale's, Burdines, Lazarus, and Jordan Marsh subsidiaries); Bonwit Teller and B. Altman; Garfinckel's; Carter Hawley Hale (including its Broadway, Emporium, and Weinstock's subsidiaries); Macy's; Ames department stores; Hills department stores; Southland (7-Eleven), Circle K, and Pay 'N Pak convenience stores; Supermarkets General; Interco (producer of Converse tennis shoes and Lane and Broyhill furniture); Continental Airlines; Eastern Airlines; Trans World Airlines; Braniff Airlines; Northwest Airlines; Heileman Brewing (the nation's third biggest beer producer); Vintner's International (Paul Masson and Taylor wines); publishers McGraw-Hill (Maxwell), Harcourt Brace, and Ingersoll; textile firms J. P. Stevens, Burlington, West Point-Pepperell, and Fieldcrest Cannon; Jim Walter Corporation; National Gypsum; U.S. Gypsum; Days Inn; Revco drug stores; Seaman Furniture; and Rexene.

Inconveniently caught in the maelstrom have been a number of the case studies Yago extols as illustrative of the benefits of junk debt. He cites food processor Dorskocil as a firm whose success ostensibly stemmed from its "access to appropriate funding"—yet Dorskocil was forced into bankruptcy in 1990, after proving incapable of meeting the interest payments on the huge debt burden it took on in a wild acquisition spree (including its purchase of Wilson Meats, a firm six times larger than Dorskocil at the time of its takeover).¹⁶

He cites Macy's as an example of how "systematic empirical research shows that these transactions [leveraged buyouts] can provide beneficial contributions to corporations and the economy" (149–50, 160). However, Macy's declared bankruptcy in January 1992, unable to service the multibillion dollar debt burden produced by its leveraged buyout six years earlier.

Kinder-Care, another "high yield" firm praised by Yago, is especially notable. Here, he claims, is a firm that employed junk debt in order to seize "opportunities created by fundamental changes in society" (i.e., child day-care service). But as carefully dissected by

analyst Benjamin Stein, the Kinder-Care story is considerably different, as are the appropriate lessons to be drawn from it.¹⁷ Once integrated into Drexel Burnham's junk-bond machine, Kinder-Care became a turbocharged, multibillion-dollar diversified financial-services firm, and a link in Drexel's daisy chain of junk: it used Drexel junk financing to raise funds, then purchased junk bonds issued by Drexel Burnham in other corporate deals. Savings and loan firms acquired by Kinder-Care also invested heavily in Drexel-issued junk. Although stockholders' and depositors' funds were used to purchase Drexel securities, Drexel paid hundreds of thousands of dollars in personal "commitment fees" directly to top executives of Kinder-Care. After these same executives acquired personal control of a shoe-store chain (in part using Kinder-Care corporate funds), they arranged for Kinder-Care to purchase their chain—personally providing themselves a tenfold profit on their investment. They also diverted corporate funds to purchase for their personal friends a publisher of deer hunting magazines and a brokerage firm.

By 1989, Kinder-Care's junk-bond holdings were hemorrhaging. By early 1991, the parent holding company was in bankruptcy. By April 1992, one top executive of the firm was in prison, and another was convicted of illegally looting stockholders' assets. In the end, Stein concludes that "the day-care chain that was supposed to serve as an example of how capitalism can adapt to social needs and serve the people while making money for the stockholders stood for something very different: basic fraud and betrayal of trust."

Another noteworthy example is First Executive Life Insurance Company, portrayed by Yago as an "entrepreneurial" insurance firm "challenging the more established names in the insurance industry" and outperforming its hide-bound, stodgy rivals by boldly investing in junk securities (182, 195). Here, again, the complete story is revealing.¹⁸ Under the now notorious direction of Fred Carr, First Executive's insurance business exploded from \$700 million in 1974 to \$10 billion by 1984. It became one of the largest buyers of junk bonds issued by Drexel Burnham, participating in as many as two-thirds of Drexel's junk offerings, and carrying an \$8 billion portfolio of junk by 1990. Buying into Drexel's junk deals temporarily proved to be an extremely lucrative business for First Executive: pension funds of firms involved in Drexel deals were stripped of assets and replaced with annuity retirement contracts

purchased from First Executive. Reinsurance “protection” for these pensions was provided by firms which turned out to be half owned by Drexel and First Executive, and which were themselves carrying portfolios packed with Drexel junk.

By 1989, First Executive began to lose hundreds of millions of dollars. By 1990, the firm defaulted on its financial obligations. In April 1991, First Executive filed for bankruptcy and was seized by state insurance officials—imperiling tens of thousands of pensions in the process. In early 1992, Carr and Milken were sued for fraud by state regulatory authorities. The country’s biggest insurance-company bankruptcy to date, some analysts consider First Executive “a startling drama of fraud, incompetence, and tragedy.”¹⁹

Nor were junk bonds a boon for small businesses otherwise denied access to credit—another touchstone of junk-debt propaganda. One study of small firms financed by junk bonds found that 24 percent of them subsequently filed for bankruptcy, and at least 40 percent of them floundered, rather than flourished, as a result of junk-bond financing. Observed one analyst, “Drexel talked a lot of companies into believing that driving 80 miles per hour down a mountain road with no shoulder was OK. But when they did they went off the cliff.”²⁰

Of course, not every junk-bond deal failed. But as the old saying goes, it ain’t what you pray for, it’s what you bet on, that counts. Junking corporate America proved an exceedingly poor bet—for all but the small coterie of dealmakers who finagled fabulous wealth for themselves, at least for a while.²¹

The Recession Legacy

The fallout of a decade of junk-debt deals continues to compound the country’s recessionary malaise. Consumers—fearing layoffs by firms “restructuring” to atone for the follies of the eighties—are loath to spend. Banks and financial institutions, traumatized by the collapsed corporate deals in which they invested, are paranoid about lending. And overleveraged corporations, saddled with staggering debt loads, are virtually precluded from undertaking the new investment projects desperately needed to help pull the economy out of recession. The explosion of corporate debt has decimated the creditworthiness of American firms, rendering them too

leveraged to borrow for new investment in plants, equipment, or product development. At the same time, crushing interest payments required to service debt loads continue to siphon off the corporate cash flows that otherwise would be available for internally funding new investment projects. Compounding the economic instability of excessive debt, the proliferation of what were euphemistically called “innovative high-yield securities” — accrued-interest junk bonds, balloon-payment junk bonds, zero-coupon junk bonds, payment-in-kind junk bonds, “reset” junk bonds — proved almost perfectly designed to amplify, accelerate, and exacerbate the adverse recessionary forces that subsequently developed.

The Opportunity Cost

More damaging to the economy over the longer run is the massive opportunity cost inflicted by a decade of corporate deals. The vast sums spent shuffling paper ownership shares during the 1980s are, at the same time, vast sums that were *not* invested in new plants, new research and development, new product commercialization, new jobs, or the development and implementation of new state-of-the-art manufacturing techniques. In 1986, for example, corporate America spent more on mergers and acquisitions (\$204 billion) than it did for research and development (\$56 billion) and net new plant investment (\$81 billion) *combined*. More generally, the trillion dollars devoted to corporate deals during the decade represents a trillion dollars *not* plowed directly into the nation’s industrial base.

During the 1950s, American firms invested \$3 billion building new plants and equipment for every \$1 billion paid out in interest to service their debts. During the 1980s, that relationship was reversed: American corporations were expending \$1.6 billion in interest payments for every \$1 billion invested in plant and equipment. Similarly, during the 1950s U.S. firms allocated \$710 million for research and development for every \$1 billion they paid in interest on debt. During the 1980s, however, that ratio plummeted 70 percent (to \$220 million spent for research and development for every \$1 billion in interest payments).²²

While junk-bond jobbers, dealmakers, and corporate America devoted themselves to the art of the deal, America’s toughest global rivals poured their funds into new plants, new products, and new

research and development. During the 1985–1988 period, Japanese firms increased their expenditures for factories, equipment, and research and development by an estimated 150 percent—compared with an increase of only 23 percent by American firms. By 1988, annual Japanese corporate investment exceeded that of American firms in absolute dollar amounts, despite the fact that the U.S. economy is 40 percent larger than Japan's. On a per-capita basis, Japanese firms out-invested American firms by a daunting two-to-one margin.²³

Obviously, the challenge of closing this industrial-technological gap will be even more difficult, require even more effort, and take even longer—another legacy of a lost decade of corporate deals.

Roaring Twenties Redux

Finally, there are the striking similarities between the debt-financed deal mania of the 1980s and its counterpart of the 1920s.²⁴

In the twenties as in the eighties, a frenzy of debt and speculative corporate dealmaking ensnared the American economy. Then, as in the eighties, “experts” praised financial magicians for discovering the key to permanent prosperity: highly leveraged corporate structures wildly thrown together, they were sure, “made for speed, for efficiency, for harmony . . . did away with red tape,” and put the affairs of firms into “the most capable and deserving hands.” Then, as in the eighties, corporate dealmakers were praised for what an Ohio State University economist called “their vision for the future” and for boldly breaking free from “the heavy armor of tradition.” Then, as in the eighties, stern warnings were sounded against government interference which, it was threatened, would “deny investors the means of recognizing economies which are now proved, skill which is now learned, and inventions which are almost unbelievable.”²⁵

And of course, then, as now, the house of cards collapsed. An analysis of the subsequent debacle published in 1933 by the Twentieth Century Fund, and its assessment of the role played by debt and corporate deals, merits quotation:

The far-flung holding company systems had a mushroom growth during the decade before the stock exchange collapse in 1929. Their development had been lavishly lauded because of the proclaimed economies in manage-

ment, which were promised through consolidation and coordination, through system planning, and through centralized financing. . . . The basic weakness in the financial structures was the overpayment for the properties and the excessive fixed charges which resulted from the combinations. The purchases were made largely under competitive bidding between holding company groups. The prices paid were not only high enough to induce the local owners of the properties to sell, but also to out-bid other competitive groups.

Even under normal circumstances, the holding company systems would probably have found themselves in difficulty to support the extravagant financial structures which were erected. . . . But with the financial collapse of 1929, the situation was rapidly changed.²⁶

Instead of marking a new development, the junk-bond eighties represent the latest outbreak of a recurrently exhibited, extensively documented propensity for financial lunacy on a massive scale.

* * *

Baron James de Rothschild once observed that a "man who is in constant monetary straits, stops up one hole while making another and who is compelled to execute a perpetual egg dance among more or less dangerous debit balances, will, in the end, after every fresh success in averting imminent catastrophe, think himself a financial genius." Perhaps the visionary disciples of junk-debt deals were distracted by the egg dance of the 1980s. But they seem to have lost sight of the crucial distinction between productive capitalism and speculative capitalism. One expands what Adam Smith called the real wealth of a nation; the other merely redistributes existing wealth. One builds factories; the other redistributes ownership of existing plants. One gives birth to new goods, services and production techniques; the other merely rearranges control over them. One contributes to economic growth and productive job creation; the other is a destructive zero-sum game.²⁷

The consequences, events show, are not insignificant. Nor are they short lived.

NOTES

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2. Walter Adams and James W. Brock, *Dangerous Pursuits: Mergers and Acquisitions in the Age of Wall Street* (New York: Pantheon Books, 1989), 11-12.
3. *Economic Report of the President*, 1989 and 1992 (Washington, D.C.: U.S. Government Printing Office).
4. *Business Week*, January 21, 1991: 29.
5. Donald Barlett and James Steele, "Pushing for Answers," *Columbia Journalism Review* (March/April 1992): 32.
6. Andrew F. Brimmer, president, Brimmer & Company, Inc., "Innovation and Regulatory Restraints in the Corporate Bond Market." Paper presented at Eastern Economic Association meetings, Cincinnati, March 30, 1990.
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8. Gertler and Hubbard, 3.
9. For one analysis, see Benjamin Stein, "Junk Bunk," *Barron's*, July 1, 1991: 14. For another critique, see U.S. Congress, House, Subcommittee on Science, Research and Technology, Hearing: Corporate Restructuring and R&D, 101st Cong., 1st sess., 1989, 58-79 (testimony of William F. Long and David J. Ravenscraft).
10. Walter Adams and James W. Brock, *Antitrust Economics on Trial: A Dialogue on the New Laissez-Faire* (Princeton: Princeton University Press, 1991), 104.
11. Stratford P. Sherman, "Bankruptcy's Spreading Blight," *Fortune*, June 3, 1991: 123.
12. James A. White, "Total of Defaults for Junk Bonds Soared Last Year," *Wall Street Journal*, January 9, 1991: B2; Matthew Winkler, "Junk Bond Market Is Seen Showing 38% Default Rate," *Wall Street Journal*, January 25, 1990: C20; Jeremy C. Stein, "What Went Wrong with the LBO Boom," *Wall Street Journal*, June 19, 1991: A10.
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14. Steven N. Kaplan and Jeremy C. Stein, "The Evolution of Buyout Pricing and Financial Structure," National Bureau of Economic Research Working Paper No. 3695, May 1991.
15. Grant, 431.
16. Karen Blumenthal, "Dorskocil Files for Chapter 11 Protection," *Wall Street Journal*, March 6, 1990: A4.
17. See Benjamin Stein, "No Child's Play: How Once-Promising Kinder-Care Was Laid Low by Financial Fraud," *Barron's*, April 20, 1992: 8.
18. See Benjamin Stein, "Sunk by Junk," *Barron's*, April 1, 1991: 8; Frederick Rose and David B. Hilder, "Mounting Junk Woes Imperil First Executive and Chief Fred Carr," *Wall Street Journal*, January 29, 1990: 1; Frederick Rose and David Wessel, "Junk-Bond Woes Put Retirement Benefits in Danger for Many," *Wall Street Journal*, February 12, 1990: 1; Frederick Rose and

David B. Hilder, "Junk-Bond Deals Trip Insurer as State Seizes Unit of First Executive," *Wall Street Journal*, April 12, 1991: 1; Peter Applebome, "Mill Town Pensioners Pay for Wall Street Sins," *New York Times*, July 30, 1991: 1.

19. Stein, "Sunk by Junk."
20. Michael Selz, "Did Junk Bonds Help Small Businesses?" *Wall Street Journal*, November 20, 1990: A12.
21. Some contend that the raids and hostile takeovers of the 1980s served to streamline corporate America by forcing firms to divest ineffective operations, and by transferring control to better management. Three considerations undermine this argument, however. First, record bankruptcy rates are scarcely indicative of superior managerial prowess. Second, many of the divested operations were ill-advised acquisitions, which the antitrust laws could have prevented in the first place had they been enforced. Third, the premise of the argument is that product markets massively fail to promote good economic performance, in which case vigorous antitrust enforcement may be a far more preferable remedy than financial ruination via deals and debt. See Adams and Brock, *Dangerous Pursuits*.
22. Barlett and Steele.
23. Carla Rapoport, "Japan's Capital Spending Spree," *Fortune*, April 9, 1990: 91-92; David E. Sanger, "Japan Keeps up the Big Spending to Maintain Its Industrial Might," *New York Times*, April 11, 1990: 1.
24. For a detailed comparison, see Adams and Brock, *Dangerous Pursuits*, 132-39.
25. Quoted in John Kenneth Galbraith, *The Great Crash: 1929* (Boston: Houghton Mifflin, 1954), 19, 44.
26. E. Clark, ed., *The Internal Debts of the United States* (New York: Twentieth Century Fund, 1933), 157.
27. For a comprehensive examination of this distinction and its public policy significance, see *Dangerous Pursuits*, esp. chs. 12-15.

GOVERNMENT INTERVENTION:
SOURCE OR SCOURGE OF
MONETARY ORDER?

Charles Kindleberger argues that most, if not all, financial manias, panics, and crashes were market failures deriving from the irrational behavior of human actors. Both his notion of rationality and his interpretation of the sources of financial crises are open to question. A broader notion of rationality enables us to distinguish actual crises from cases of fraud or entrepreneurial error, and a closer look at financial history illustrates the ways in which government regulation, not human irrationality, has been the source of financial disorder.

One of the few things that both defenders and critics of the market agree on is the importance of money in forming and describing modern social orders. Both friend and foe pay money the highest tribute by continually reemphasizing its catalytic role in the ways we live and work together. Despite this recognition of money's importance, far less attention is paid to the institutions that are involved in the production and distribution of money and other financial products and services. Indeed, given the central role of this framework of institutions in ensuring, and occasionally disrupting, the flow of goods and services in market economies, both sympathetic and critical observers should have more than a passing

interest in the theory and history of financial markets and institutions.

One critic who has not neglected their importance is the economic historian Charles Kindleberger. Kindleberger's *Manias, Panics and Crashes*, rev. ed. (New York: Basic Books, 1989) attempts a comprehensive survey of various financial crises throughout modern Western history.¹ The message that emerges from his impressive command of financial history is that financial institutions predicated on assumptions of human rationality are likely to be rendered unstable by the whims and vagaries of human passions, irrationality, and mass psychology. Consequently, government-owned or -directed central banks have played a crucial role as lenders of last resort in preventing and mitigating financial crises brought on by the insufficient rationality of human beings and spontaneously organized economic institutions: "Central banking arose to impose control on the instability of credit. The development of central banking from private banking, which is concerned to make money, is a remarkable achievement" (85).

Despite Kindleberger's breadth of knowledge, this conclusion is questionable because the focus of the supporting analysis is frequently too limited. The problem begins with an overly narrow conception of rational behavior. If we adopt a more contextual notion of rationality (one that includes the institutional environment in which action occurs), then action that Kindleberger wants to call irrational can still be rendered intelligible, although not necessarily rational in the strongest sense. Given this modified version of rational behavior, the undesirable social consequences of manias, panics, and crashes can be seen to emerge from the instrumentally rational behavior of financial actors within the irrational financial institutions that have historically resulted from misguided and self-interested attempts at state intervention.

The Contexts and Consequences of Rationality

Kindleberger begins by asking, "Are markets so rational that manias—*irrational by definition*—cannot occur?" (4, emphasis added) This is not a promising start. If financial crises are irrational by definition, they would appear to be ill suited to systematic or theoretical explanation. It simply is not clear here, or elsewhere, exactly

what Kindleberger means when he uses the words “rational” and “irrational.” At times he uses them to describe individual action (e.g., 5, 19, 30, 32), at times he describes market *outcomes* this way (e.g., 10, 31, 33), and sometimes they describe markets themselves, as in the quotation above.²

The general sense of all three usages, however, seems to be that in manias, panics, and crashes, the behavior of economic actors is inconsistent with economic realities. For example, suppose a rumor concerning the financial health of IBM emerges. If the rumor is ultimately unfounded, but actors sell off IBM stock and take other actions inconsistent with the underlying state of affairs, their actions are “irrational” on all three levels: the behavior itself is irrational, the outcome is irrational (if IBM stock takes an unwarranted dive), and the market is irrational for enabling such an event to occur.

On the first level, Kindleberger’s view of irrationality seems to boil down to erroneous behavior. Without quibbling over definitions, one may note that Kindleberger implies that virtually all human action is, by virtue of being undertaken by imperfect beings in an uncertain world, irrational. According to less rigorous usage, one remains rational even if, given the available information, one’s decisions prove to be erroneous. Why does Kindleberger stray so far from the usual understanding of rationality?

In doing so, I think, he takes up a position as extreme in its own way as the opposite view of rationality adopted by the New Classical macroeconomists whose empirical arguments Kindleberger seeks to defeat. Rational expectations theorists, such as the New Classical economists, assume that the subjective probability distributions of agents’ expectations are precisely equal to the “objective” probability of the occurrence of possible future events. Agents make the best possible guess about the future they can because they are assumed to possess all of the relevant information. If one adopts this assumption, any observed outcome in the market is not only rational, but optimal. Note that on any given occasion actors might be wrong, but they will be optimally wrong in that, on average, they could have done no better. According to rational expectations theory, however, actors can never be *systematically* mistaken because if there is something systematic taking place, that is a piece of information actors will be assumed to possess, enabling them to avoid the error. In this view, manias are “rational” in that they

simply reflect the utility-maximizing decisions of agents with rational expectations. The implication is that there is nothing that policymakers can do to improve things because their actions will be built into the expectational frameworks of agents who are determined to act "maniacally."³

Definitions aside, this view of rationality includes empirical assumptions that Kindleberger is quite right in rejecting. To assume that actors have all the relevant information overlooks the whole problem of precisely how economic knowledge is discovered, dispersed, and socially utilized.⁴ However, we need not immediately conclude that manias are necessarily irrational (in any nontautological sense). Between Kindleberger's leap to irrationality and the New Classical faith in a strong sense of rationality there must be the fabled middle ground.⁵ One can reject the strong epistemological assumptions of rational expectations theory, yet still see economic actors as being rational in the sense that they will make the best decisions they can from their point of view, with the assistance of the bits and pieces of information provided by such market phenomena as prices and profits. In such a world, over time and in general, the behavior of economic actors will lead to increasing degrees of economic order as market institutions provide information about the correctness of specific actions as well as guidance as to how to improve them.⁶

One advantage of this view of rationality is that it pays attention to the institutional environment in which action occurs. Economic action will be more rational (or correct) in an environment where economic and social institutions are better at providing feedback and processing knowledge. If there is a significant number of mistaken actions in a given market, then we might ask whether an alternative institutional structure might evoke fewer mistakes. The U.S. savings and loan crisis might be a good example. Why did so many S & Ls have so many problems at the same time? Was it irrationality or fraud by owners and managers, or did institutional incentives make mistakes more likely? If the latter, then owners and managers may have been perfectly rational given the institutional incentives, and calling this episode an example of market, or individual, irrationality would seem to miss the empirical factors involved.

Fraud, Entrepreneurial Error, or Crisis?

In fact, despite his claims that manias, panics, and crashes are irrational, Kindleberger has a theory to explain them. Borrowing from economist Hyman Minsky, and some classical predecessors, Kindleberger's model attempts to portray a "typical" crisis (17-27). The sequence of events begins with some sort of "displacement" (17) that alters profit opportunities. This displacement is enhanced by credit expansion. As firms see new profit opportunities they demand more loans, and banks, presumably seeing similar opportunities, acquiesce and provide them (18). Kindleberger notes that the influx of new credit can also come from new banks opening up.⁷

The fire, having been started, is fueled by "overtrading" (19). Kindleberger admits there is no clear definition for this term, but it seems to be another version of the notion that current economic activity is inconsistent with "real" factors. Pure speculation and buying on margin are mentioned as possible examples of such behavior, which culminate in Kindleberger's "mania." At some point, the speculative bubble bursts. Perhaps insiders begin to take profits, or some firm fails. Whatever the cause, it reveals the mania's excesses and the "panic" stage occurs. Each speculator is trying to sell off their acquisitions before everyone else so as to avoid capital losses (21). This leads to a liquidity rush, where the unavailability of money is itself a source of further panic (22). If no appropriate actions are taken, primarily if there is no lender of last resort to provide the needed cash, the panic will turn into a crash as the lack of money brings exchange to a halt.

Although there are a number of specific problems with Kindleberger's model, in this section I would like to sort out a more global problem. Even though a variety of economic phenomena might be thought to be explained by the model, not all such phenomena imply the same solution. Kindleberger could be describing a business cycle generated by central bank policy, a fraudulent investment scheme that takes in a large number of people, or widespread entrepreneurial error due to institutional or cultural factors.

We can refer to these three types of events as "crisis," "fraud," and "error." Frauds, of course, have occurred throughout economic history. However, their relationship to financial crises is not at all clear. Take one of Kindleberger's favorite examples, the classic Ponzi scheme. Investors are attracted by promises of large interest

payments for invested sums. Money is taken in, and the interest, if any is paid, is taken from the principal of later investors. The schemer tries to stay one step ahead of his promises until suspicion is wide enough that he is either caught or makes a getaway. How such a scheme could throw an entire economy into crisis is not clear. On a large enough scale, a Ponzi scheme might have some economywide effects. However, in an era of rapid communication and investigative journalists, such a scenario seems farfetched.

Moreover, the parsimonious remedy for fraud is surely independent of the banking system: stronger legal protection or punishment for the crime. Although the financial system should not facilitate such activities, it is a mystery how Kindleberger's several examples of fraud show the need for a central bank/lender of last resort.

As for error, entrepreneurs are going to make mistakes. That is part and parcel of any economic system that faces radical uncertainty. If entrepreneurs pursue potential profits where none exist, they will incur losses. In so doing some firms may face bankruptcy, and others may get nervous about other investments. Indeed, some sort of aggregate downturn might occur. But in one sense such downturns are beneficial. One of the most important benefits of markets is that they provide feedback to participants. Sometimes entrepreneurs may "overtrade" or bid stock prices to levels that ex post are seen to be too high. The problem is that ex ante there is no way of knowing with certainty whether one's actions are appropriate or not. Markets provide this knowledge, ex post, by attaching economic losses to them. This is crucial if errors are to be corrected, so that even greater losses (and their human costs) are avoided.

Suppose investors think a mining firm will be able to profitably exploit some newly found ore. They will drive up stock prices. Now suppose the ore is found to be prohibitively expensive to extract. Stock prices will fall. Assuming (which one can never know with certainty ex ante) that the falling price better reflects real factors, it has revealed entrepreneurial error. No economic regime can completely eliminate the possibility of such error; a regime of competitive investors may minimize it. In any case, it is far from clear how central banks could play a significant role in minimizing such error.

What I have termed "fraud" and "error" are consistent with Anna Schwartz's distinction between "pseudo-financial crises and real

financial crises.”⁸ For Schwartz, the distinction rests on whether or not the banking system is able to provide the necessary liquidity in the face of a dramatic switch from bank liabilities (deposits or, before the advent of central banking, currency) to high-powered money (central-bank currency or, previously, gold). In the context of American banking today, Schwartz’s definition of a financial crisis would refer to the inability to convert one’s deposit account into cash because the bank did not have enough currency to meet the demand. On her account, for example, the bank failures between 1929 and 1933 constituted a real crisis, as banks in fact did not have enough cash to meet withdrawal requests, while the 1987 stock market drop would be a pseudocrisis as the Fed intervened to provide the liquidity. What is useful about this definition is that it distinguishes between the economic crises that are caused by the financial system and those that are not. Kindleberger’s response to this line of argument is telling: “We dismiss without argument the monetarist position . . . that a distinction should be made between ‘real’ (or ‘true’) and ‘pseudo’ financial crises” (23). His refusal to address distinctions among his various forms of crisis lets him suggest solutions to nonfinancial crises that would, even accepting his arguments for them, apply only to financial crises.

The cause of real financial crises must lurk within the institutional structure of the banking system. If financial crises are characterized by an inability to turn bank liabilities into high-powered money, under what circumstances is this problem likely to occur? What is it about the banking system that is preventing the proper amount of high-powered money from being produced? To what degree has government intervention in the banking system made these problems more or less likely to occur? As we turn to explore some of Kindleberger’s specific historical examples, we need to keep this last question at hand so as to determine whether the crises he discusses are the fault of the inherent irrationality of markets, as implied by his suggestion that a central bank/lender of last resort is the institutional solution.

Central Banking and the Money Supply

In Kindleberger’s view, monetary expansion is a key part of the mania, panic, and crash sequence. By increasing the supply of

money and credit, the banking system fuels a mania that is already underway. So Kindleberger is prompted to discuss the theory and history of attempts to ensure that monetary institutions are not agents of inflationary expansion. Of specific interest is the debate between the so-called Currency and Banking schools in early nineteenth-century Britain.

Members of the Currency school—precursors of today's monetarists—argued that, given the existence of a central bank, there should be statutory limits on how much currency it could produce. Kindleberger argues (58) that although limits on currency production were desirable, the Currency school and the law that embodied its ideas (the Bank Act of 1844) should have also limited deposits. Members of the Banking school, by contrast, argued that there was no need for statutory limits. Their view was that the quantity of money was endogenous and driven by the “needs of trade.” As commercial activity increased, the demand for bank credit would rise and banks would accommodate the demand by creating additional money. In the Banking school view, all banking systems automatically adjusted the supply of money to the needs of trade.

Kindleberger is one of many to note that both views are problematic. The Currency school's view forces one to define precisely what is and what is not money. Without such a definition, it is not clear what variable is to be limited. The problem with defining money is that limits based on such a definition are liable to generate money substitutes. Kindleberger argues that “the process is endless: fix any M_i and the market will create new forms of money in periods of boom to get around the limit and create the necessity to fix a new variable M_j ” (63). Others have argued that such limits might be fine until they were tested. Once it was known that the currency supply was near the limit, the existence of the limit might lead to a scramble for cash as actors see that no more will be forthcoming.⁹ As we shall see, many past financial crises were linked to government interventions that prevented banks from keeping the supply of money, especially currency, coordinated with changes in demand.

The Banking school view is equally flawed. First of all, is it true that all banking systems will automatically produce the amount of money demanded? Many critics have pointed out that the “law of reflux” (the idea that excess money would flow back to the issuer)

that was relied on to ensure the proper money supply is far less effective in systems with a monopolized currency.¹⁰ Because monopolized currency is the only form of paper money available, there are powerful incentives for actors, particularly banks, to hold stocks of it regardless of how much is being issued. The only way excess supplies can be made known to a monopoly issuer is if currency flows out of the country and is eventually redeemed for the commodity “backing” the currency—if one exists. This is what happened during the late 1960s, when U.S. inflation led to outflows of gold, prompting President Nixon to “close the gold window”—that is, remove the commodity backing from the dollar, which allowed the continued issuance of excess currency.¹¹

Similarly, if banks are able to supply “the needs of trade,” there is reason to expect temporary monetary instability to be aggravated, as banks comply with the loan demands of businesses that have been artificially stimulated by excessive credit, exacerbating the stimulus.

Although he sees the flaws in both views, Kindleberger’s alternative seems equally dubious. Since he equates the failures of the Currency and Banking schools with the failure of free banking—that is, banking without a legally imposed central bank—he must rely on the skill of central bankers to act in the right way at the right time: “The role of a lender of last resort is fraught with ambiguity and dilemma . . . intervention is an art, not a science” (4–5). One searches in vain for any specific policy or institutional design that might overcome the obvious difficulty that, with central banking a reality for most of this century, it can hardly be said to supply a remedy for “manias, panics, and crashes.” Kindleberger’s reliance on the skill of human central bankers is particularly problematic as he spends so much of the book documenting what he sees as the inherent irrationality of human behavior and our susceptibility to mass psychology and manias. Why should we believe that the humans who staff central banks are any more rational than those who run wild in the markets the banks regulate?

The problem is that neither the financial system described by the Currency school nor that of the Banking school is, as Kindleberger assumes, a market regime. In such a regime, consumers would almost certainly patronize currencies that gave some promise of not being overissued. One way to make such a promise is to back the currency (and any deposits) with a commodity, such as gold, or

with other marketable financial assets. Under a market regime of this sort, an overissue of currency or deposits would flow back to the issuing bank, draining it of its redemption medium and posing increasing risks to its viability. Unlike a monopoly overissuer of currency (e.g., an inflationary central bank), however, a currency issuer in a legally unrestricted market could not “close the gold window” without sending a signal about its problems, and therefore driving its customers to the competition. Under such a system, the reflux of excess currency that Banking theorists erroneously thought would occur in *any* banking system could take place. Yet Currency school-type limits on the money supply would be obviated by market incentives for banks not to overproduce money. On the other hand, “free banks” would be unlikely to *underproduce* money, since doing so would sacrifice the interest they could earn by lending as much money as the demand for loanable funds at prevailing interest rates would allow.

Free banking would, like central banking, ultimately depend on the judgment of fallible human beings as to the demand for money. But rather than a legally entrenched, politically influenced, single group of human beings, such a system would empower those whose judgment had proven superior to that of their competitors—in short, those who enjoyed success in their vocation. Bankers who successfully gauged the demand for money would be able to reap the greatest return from extending credit while avoiding the loss of the redemption medium that buttressed their monetary liabilities. In short, their financial interests would be congruent with avoiding manias, panics, and crashes. If we find, by contrast, that financial crises recur with frequency and regularity, it may well be that we can look for an explanation to the legal restrictions that prevent free banking in favor of a central bank monopoly—rather than assuming, as Kindleberger must, that people are prone to irrationality, with the glaring exception of central bankers.¹² Having established free banking as an ideal-typical contrast to government-controlled or -regulated banking, then, we can assess Kindleberger’s attribution of panics and crashes to “manias.”

A History of American Banking Regulation

From its earliest days, American banking was, by Kindleberger's account, severely underregulated. During the so-called Free Banking Era (1837-1863), he contends, those states that gave banks the greatest latitude experienced the most "wildcat banks," bank failures, and other problems. Federal regulation and oversight was needed and the National Banking System (1863-1914) was a step in the right direction. However, Kindleberger argues that because it did not go far enough and could not cope with an increasingly complex financial environment, both domestically and internationally, panics marred its record. The Federal Reserve System was created in 1914 in the hope of solving such problems, but the government was still not powerful enough until the reforms of the 1930s, such as deposit insurance and the Glass-Steagall Act's separation of banking from securities. This system worked fairly well until the 1970s and 1980s when, according to Kindleberger, changes in markets and the move toward deregulation, turned back the clock and once again left the system vulnerable to speculative excess. The solution is to design better and more sophisticated regulatory systems to match the changes of the late twentieth century; in particular there is a need for international central banking and regulation.

Kindleberger's Whiggish story of the progressive enlightenment of politicians and regulators is naïve, at best. For he overlooks how each of the crises that led to greater government regulation was caused by the regulations of the previous period and by the revenue-raising interests of the state. Like historians who attribute economic problems to a nonexistent age of "*laissez faire*," Kindleberger tends to assume that if an era was characterized by *comparatively* little federal intervention in banking, then financial problems must have been due to market rather than regulatory forces.

Thus, the wildcat banks of the mid-nineteenth century were, in fact, heavily regulated by the states. In almost every state, banks had to meet strict reserve requirements, they were very limited in their ability to open intrastate branch offices, they were prohibited from opening interstate branch offices, and they were required to purchase certain assets (usually state government bonds) to serve as collateral against their currency. The reserve requirements benefited the larger city banks, which usually held the deposits of

smaller banks. By requiring smaller banks to keep deposits at larger ones, the larger banks had increased reserves with which they could create credit and earn profits. The prohibitions on branching limited competition among banks by removing the threat that a new branch of an existing in-state or out-of-state bank would open nearby. And the collateral requirements gave state governments a captive market for their debt.

Although all of these regulations were important, the second was the one that was most relevant to wildcat banking. The limits on branching made it difficult for banks to develop interbank settlement institutions, particularly in the relatively unsettled West of the nineteenth century. Without the ability to branch, banks could not develop the sort of repeated interactions that might lead to institutionalized interbank relationships. No single bank dealt so frequently with any other particular bank, except perhaps in major cities, that it became profitable to regularize and institutionalize interbank clearing practices. Most interbank relationships were rather ad hoc. Without interbank institutions, banks could not provide themselves with any sort of governance system and therefore provide consumers with information they could use to distinguish good banks from bad. As some of these institutions developed in the bigger cities, one of their functions was to provide the public, via the newspapers, with information on the health of member banks.

In addition, the lack of institutionalized interbank relationships lengthened the time lag between the deposit of a bank's notes with a competitor and the competitor's ability to exchange them for reserves. (This problem persists in the time it still takes for an out-of-state check to clear.) Most wildcat banks operated in rural areas, as far from other banks and banking institutions as possible. By doing so they hoped to capitalize on this time lag by spending on goods and services and then closing up before holders of the currency could attempt to redeem it for gold. Prohibitions on branching made it easier to hide from other banks, and the ad hoc nature of interbank relationships made it easier for a new, unknown, banking entity to have its currency accepted by other traders or other banks. Work by economic historians has used this type of analysis to question both the causes and severity of wildcat banking.¹³

The National Banking System (NBS), which followed the Free

Banking Era, was designed with two purposes in mind. One was to create a unified national currency by creating a new class of federally chartered banks. The second was to use these new banks as a way to finance the Civil War. The NBS did both by adapting state-level regulations to federal needs. As state-chartered banks had been, federally chartered banks were required to maintain certain levels of reserves, to buy bonds (now federal ones) as collateral, and to adhere to the same limits on branching as previously. The new system also imposed a 10 percent tax on the currency issued by state-chartered banks, which made a federal charter more desirable and further enhanced the federal government's war bond market.

All these regulations combined to lead to the panics that plagued the NBS. Rather than being the kind of irrational phenomena that Kindleberger associates with the word "panic," the crises under the NBS were rational responses to irrational regulations. The typical NBS panic began in the late summer or early fall, when the demand for currency rose with the seasonal movement of crops.¹⁴ When currency demand rose, federally chartered banks that wished to increase their production of currency were forced by the regulations to purchase specific government bonds to serve as collateral. The problem was that when a large enough number of banks were bidding for the limited supply of specific bonds, their price was driven up. At a sufficiently high price, currency creation became too expensive since the interest received on loans would be outweighed by the cost of obtaining the required bonds. In other words, the regulations made the profit-maximizing behavior of banks inconsistent with an adequate supply of currency.

Kindleberger recognizes this conflict between the self-interest of the banks and the public interest of an optimal money supply (85). But Kindleberger's diagnosis of the underlying problem is faulty. Failing to recognize the role of federal regulations, he can only attribute the banks' failures to the irrationality of mania situations where banks react to short-run profitability, ignoring the underlying economic reality. However, such an explanation for why banks would have continued to underproduce currency and risk failure during the six or seven panics under the National Banking System assumes a level of irrationality well beyond mere profit-motivated myopia. The recurrence of panics under the NBS, along with the similar patterns involved in each, cries out for some systematic explanation linked to the particular institutional and regulatory

environment involved. If the NBS panics were simply the result of group myopia, why were they all so similar, and why did they occur so frequently under one set of regulations?

The particular regulations of the NBS can provide an explanation. Federal reserve requirements forced country banks to hold a certain percentage of their reserves as deposits in larger city banks, and in turn forced various city banks to place reserves with New York banks. Thus any shortage that began in the rural areas during harvest time would eventually spread to the cities through the domino effect of the pyramidal reserve structure. At the same time, prohibitions against branch banking limited the geographical scope of clearing houses to rather small areas. Although there were clearing houses in individual cities, and sometimes in larger areas, there were very few at the regional or interstate level. This led banks to hold more reserves than was economically necessary and limited interbank cooperation to husband reserves efficiently during times of crisis.

Nevertheless, as Kindleberger himself recognizes (162-164), during panics, especially those in 1893 and 1907, the clearing houses in various cities issued "clearing-house certificates" designed for hand-to-hand use by the public. These certificates were effectively currency, backed by the assets that the member banks had deposited with the clearing houses. Those assets included *but were not limited to* the officially mandated government bonds. Technically, then, these certificates were illegal since they served as hand-to-hand currency yet were backed by unapproved collateral.¹⁵ Despite the fact that clearing houses issued over \$250 million in currency substitutes, customers' losses were negligible.¹⁶ This creative circumvention of government regulation suggests that if the NBS had not prevented them from doing so legally, banks could have averted the panics by issuing the needed currency.¹⁷

Although the causes of nineteenth-century panics may escape the comprehension of many twentieth-century theorists, bankers of the day understood that the bond-collateral requirements were responsible. A. Barton Hepburn of Chase National Bank pointed out that "to purchase [the required] 2 percent bonds, [banks] will have to pay \$108 for them. By having to pay this price for these bonds to bring out circulation, the financial situation becomes aggravated rather than relieved."¹⁸ Between the panics in 1893 and 1907, a number of bankers and businessmen met in Indianapolis to draft a

report recommending various financial reforms. The report argued for the removal of the bond-collateral requirements and recommended allowing banks to issue currency based on general asset holdings rather than specific bonds.¹⁹ The report also pointed to the substantially less-regulated Scottish and Canadian free-banking systems as models worthy of emulation.²⁰

What, then, are we to think of Kindleberger's attribution of NBS panics to mass hysteria? A strong hint is provided by reading contemporary accounts, which suggest how calm and orderly these "panics" tended to be.²¹ Bank customers waiting to redeem bank liabilities were usually patient, and the atmosphere was not unlike a modern line for concert tickets. (As for "irrationality," people often mentioned their desire to remove money from particular banks because they did not trust them for various reasons, or how they intended to move funds to banks closer to their homes.) Although very real losses of jobs and wealth are attributable to these "panics" and their underlying causes, hysteria and chaos were hardly the watchwords.

The history of the Federal Reserve era that began in 1914 is a bit more straightforward. Despite all the promises that a central bank would end the problems of panics and bank failures, the worst was yet to come. To a significant degree this is due to the failure to lift the restrictions on branch banking. The vast majority of banks that failed in the 1920s and 1930s were small unit (unbranched) banks, especially in farm states.²² Kindleberger notices this (133), but draws an untenable conclusion. He argues that depreciation of various foreign currencies during the late 1920s pushed down U.S. grain prices, leading to bankruptcies and bank failures in U.S. farm states. For him this is evidence of how panics can spread across national boundaries.

This explanation cannot, however, account for the Canadian experience during the same period. Although Canadian banks closed a number of their branches, and did so at a slightly faster rate than U.S. offices were closing or failing, there was only one bank failure in Canada during the 1920s, costing depositors only \$15 million (Canadian). During the same period, 5,712 U.S. banks failed, costing U.S. depositors \$565 million, while losses to bank stockholders were an additional \$400 million.²³ Canada was even more dependent than the United States on agricultural markets, and problems there should have hit Canada harder than the United States. But

because Canadian banks could diversify their risks by branching out over disparate areas, they were not as tightly linked to the fortunes of local economies as were U.S. banks.

In light of this problem, the U.S. government's enactment of federal deposit insurance treated the symptom rather than the disease. Deposit insurance protects depositors from the effects of anti-branching regulations without solving the underlying problem, and it may make matters worse, by increasing the riskiness of bank portfolios by promising to bail out banks that fail. But throughout the twentieth century, the political power of local banks—which are so densely concentrated in each state and congressional district—has prevented the U.S. Congress from fully rescinding antibranching restrictions, for fear of being outcompeted by truly national banks. In the name of protecting individual *competitors*, we have sacrificed the benefits of true nationwide *competition*.

Was Regulation Needed?

The historical record suggests that misguided government regulation has played a much larger role in causing financial panics and crises than Kindleberger acknowledges. By itself that insight is important. However, an even more thorough search through banking history uncovers various ways in which *banks themselves* were able to monitor each other's behavior and mitigate or prevent crises by creatively using market incentives and institutions. In this final section, I will mention two such examples.

One of the most discussed topics in banking regulation and banking history is the notion of "contagion." In any banking crisis, one problem facing both banks and customers is how to prevent a possibly appropriate run on a bad bank from spreading to healthy banks. In several places Kindleberger (e.g., 30-31) describes this tendency toward contagion as the classic example of a mania. His conclusion is that only regulation, such as deposit insurance and a lender of last resort, can prevent contagion.

To prevent contagion, its source must be understood. Bank depositors frequently find it difficult to acquire bank-specific information. If one or two banks are sustaining runs, perhaps appropriately, how can a customer of a third bank know whether such runs reflect problems specific to those banks, or more systemic prob-

lems that might threaten her bank? If the depositor lacks such bank-specific information, she may be tempted to play it safe by withdrawing her money indiscriminately. To the extent large numbers of depositors make the same decision, a small localized bank run may turn into a full-fledged run on the system.²⁴ An examination of banking history reveals the ways in which banks have provided such information, or solved the problem its lack causes, without resort to the sorts of government intervention Kindleberger suggests.

One way in which banks historically provided bank-specific information was through their advertising. Before the advent of deposit insurance, it was not unusual for banks to advertise an abbreviated version of their balance sheet as well as a partial or complete list of their boards of directors and operating officers.²⁵ A glance at a *New York Times* of the early twentieth century will confirm this. During panics, banks tended to print more complete balance sheets and provide the affiliations of their board members. Some banks went so far as to advertise their willingness to accept only deposits drawn off "reputable" banks.²⁶ This made the bank *receiving* the deposit a source of information about the bank from which the deposit was drawn. In this fashion, banks sacrificed their short-term profits in order to promote the long-run viability of the banking system, precisely the opposite of the behavior Kindleberger attributes to NBS banks and manias more broadly.

Another device through which banks policed themselves was clearing houses. Clearing houses approximated lenders of last resort by providing emergency loans or issuing some additional currency during crisis. However, clearing houses did not have the monopoly that true lenders of last resort, that is, government central banks, currently do. Clearing-house currency had to pass the market test. If a particular clearing house created too much or too little currency during a crisis, it risked the health of its member banks and thus its own source of profit. Central banks have no such incentives.

Clearing houses also enabled banks to avoid the contagion problem more directly. One of their more important functions was to collect and disseminate information on member banks. During the 1893 and 1907 panics, the *Wall Street Journal* published information provided by the New York Clearinghouse Association concerning the financial situation of member banks. Large depositors, who

were more likely to read newspaper financial pages, thus had access to some amount of bank-specific information. At times clearing houses also attempted to bring member banks underneath their skirts, so to speak. By issuing their own currency and occasionally redistributing reserves among their members, the clearing houses acted as if all the member banks were one integrated firm.²⁷ This infused the weaker banks with the good name of the stronger banks and the clearing house. The stronger banks agreed to this strategy as a way to prevent contagion and protect themselves. When the crisis subsided, the clearing house withdrew and allowed the banks to continue as before. Many have pointed to the importance of clearing-house activities in preventing the 1893 and 1907 panics from becoming any worse.²⁸

One last, and somewhat older, method banks used to prevent runs and panics was the so-called option clause. Scottish banks of the eighteenth and early nineteenth centuries developed the option clause as a way to legally delay redeeming their self-issued currency until they had sufficient time to acquire reserves. To keep note holders happy in the interim, banks paid them interest until redemption was resumed.²⁹ Banks that offered option clauses were less likely to be victims of runs because customers knew the banks would invoke the option clause if a run even appeared to start. Once the clause was invoked, there was no incentive to run on the bank since the bank was contractually obligated to pay the value of its currency in gold with interest. The option clause was eventually outlawed in Scotland, partially due to the beliefs that it led banks to engage in overly risky behavior and that it worked to the disadvantage of those who wanted immediate redemption. More recent research casts doubt on the veracity of the beliefs involved.³⁰ It is also worth noting that option clauses were never legal in the United States, which may partially explain why its banking system was much more prone to panics than was the Scottish one.

Despite Kindlebeger's claim that central banks and their various regulatory interventions were necessary, the historical record suggests that those very interventions have frequently initiated or dramatically exacerbated manias, panics, and crashes. The record also suggests that banks themselves were frequently the source of stability in the face of these crises, creatively dealing with the problems caused by regulatory intervention. It is tempting to blame the ills of market economies on the market, but in the case of banking,

regulation itself seems to be at fault. It is a commonplace that supposedly laissez-faire economies are often shot through with regulatory interventions; it behooves careful scholars to consider, then, whether these interventions may be responsible for the crises that serve as the rationale for further intervention.

NOTES

1. Readers with an interest in these issues might also want to see John K. Galbraith, *A Short History of Financial Euphoria* (New York: Viking, 1993). In addition, Richard H. Timberlake's *Monetary Policy in the United States* (Chicago: University of Chicago Press, 1993) provides a nontechnical overview of the history of American banking and its crises that parallels my arguments in this paper. A more technical treatment can be found in George Selgin, "Bank Lending 'Manias' in Theory and History," *Journal of Financial Services Research* 6 (1992).
2. The reader may also want to consult Kindleberger's appendix on rationality in economics (243–48) for a brief but congenial criticism of the way many economists talk about rationality. Unfortunately his usages throughout the main text are not as clearly delineated as is his discussion in the appendix.
3. Robert Lucas's classic "Econometric Policy Evaluation: A Critique," in idem, *Studies in Business Cycle Theory* (Cambridge: MIT Press, 1981), was crucial to the recognition that the expectations of economic actors are not invariant to the regimes and actions adopted by policymakers. But accepting the notion that actors will choose differently given different policies and institutions in no way necessitates adopting the considerably stronger assumption of rational expectations theory. Saying actors will consider policy does not imply that they do it with perfect rationality.
4. F. A. Hayek, "The Use of Knowledge in Society," in idem, *Individualism and Economic Order* (Chicago: University of Chicago Press, 1948).
5. The metaphor of the middle ground on the issue of rationality is borrowed from Roger Garrison, "Austrian Economics as the Middle Ground: Comment on Loasby," in Israel Kirzner, ed., *Method, Process, and Austrian Economics* (Lexington, Mass.: Lexington Books, 1982).
6. Steven Horwitz, *Monetary Evolution, Free Banking, and Economic Order* (Boulder: Westview, 1992), ch. 2.
7. Kindleberger is mistaken in believing that the opening of new banks will add to monetary expansion. Those new banks need reserves to back their liability issues and, unless they can pull currency out of people's mattresses, those reserves can only come from the reduction of other banks' reserves and, therefore, their production of money, or by convincing the central bank to increase the supply of currency or bank reserves. Without

- the help of a central bank, new banks cannot be sources of monetary expansion. See Selgin, *ni* above, 172-74.
8. Anna Schwartz, "Real and Pseudo-financial Crises," in Forrest Capie and Geoffrey Wood, eds., *Financial Crises and the World Banking System* (London: Macmillan, 1986), 11-31.
 9. As Hayek said, "I would not like to see what would happen if under such a provision it ever became known that the amount of cash in circulation was approaching the upper limit and that therefore a need for increased liquidity could not be met." See his *Denationalisation of Money*, 2nd ed. (London: Institute for Economic Affairs, 1978), 77.
 10. Lawrence H. White, *Free Banking in Britain* (Cambridge: Cambridge University Press, 1984), 127; and David Glasner, "On Some Classical Monetary Controversies," *History of Political Economy* 21, no. 2 (Summer 1989): 211-218.
 11. At that time the redemption of Federal Reserve notes into gold was only available to non-U.S. citizens. Americans lost that right in 1934 when President Roosevelt prohibited public possession of monetary gold.
 12. Kindleberger dismisses free banking by pointing to supposedly contradictory historical evidence. In particular, he cites "the country banks in England from 1745 to 1835, wildcat banking in Michigan in the 1830s, and the latest experience with bank deregulation in Latin America" (82).
 13. See the classic essays by Hugh Rockoff, "The Free Banking Era: A Re-examination," *Journal of Money, Credit, and Banking* 6, no. 2 (May 1974); and Arthur Rolnick and Warren Weber, "The Free Banking Era: New Evidence," *American Economic Review* 73, no. 5 (December 1983).
 14. See, for example, A. Piatt Andrew, "The Influence of the Crops upon Business in America," *Quarterly Journal of Economics* 20, no. 2 (May 1906). Kindleberger recognizes this phenomenon (119), but fails to see its significance in relation to the bond-collateral requirements, as discussed below.
 15. See A. Piatt Andrew, "Substitutes for Cash in the Panic of 1907," *Quarterly Journal of Economics* 22, no. 3 (August 1908); Richard Timberlake, "The Central Banking Role of Clearinghouse Associations," *Journal of Money, Credit, and Banking* 16, no. 1 (February 1984); and Steven Horwitz, "Competitive Currencies, Legal Restrictions and the Origins of the Fed: Some Evidence from the Panic of 1907," *Southern Economic Journal* 56, no. 4 (January 1990).
 16. See Andrew, *ni5* above, various pages, and Timberlake, *ni5* above, 13.
 17. For more detail on the 1893 and 1907 panics from this perspective, see Horwitz, *n6* above, ch. 5.
 18. See *New York Times*, November 12, 1907: 2, as cited in *ibid.*, 152.
 19. J. Lawrence Laughlin, *Report of the Indianapolis Monetary Commission* (Chicago: University of Chicago Press, 1898), 277-340.
 20. Interestingly, Kindleberger notes (130) that Canada seemed less susceptible to the U.S.-type panics, but never links this to the fact that the Canadian system lacked bond-collateral requirements and limits on branching, arguably the two sources of U.S. instability (see below).

21. Horwitz, n6 above, 156–170.
22. Bert Ely, “The Big Bust: The 1930–33 Banking Collapse—Its Causes, Its Lessons,” in Catherine England and Thomas Huertas, eds., *The Financial Services Revolution: Policy Directions for the Future* (Boston: Kluwer, 1988), 52. When over 4,801 banks failed between 1930 and 1933, only 8 percent of the banks over \$10 million in size failed, while the comparable rate for banks under \$250,000 was 31 percent. This higher failure rate for smaller banks is consistent with the antidiversification effects of branch banking restrictions.
23. *Ibid.*, 54–55. Ely also notes that even despite the contraction, Canadian banks “continued to maintain offices in more relatively small towns and villages [compared to the United States], particularly after 1930.”
24. Of course systemic runs are proportionally worse because in a localized run the banks may get help from other banks. If the whole system is sustaining a run, there may be no other place from which to acquire reserves, and banks may have to temporarily suspend or limit withdrawals, exacerbating an already bad situation.
25. With deposit insurance, the need for such information is obviated. If their deposits are insured by the government, banks have no reason to pay to provide such information because it does not matter to depositors. No matter where one deposits money, the risks are essentially the same.
26. See the advertisement of Winmill and Fish Bankers in *New York Times*, October 25, 1907: 15.
27. See Timberlake, “The Central Banking Role of Clearinghouse Associations”; and Gary Gorton and Donald J. Mullineaux, “The Joint Production of Confidence: Endogenous Regulation and Nineteenth Century Commercial-Bank Clearinghouses,” *Journal of Money, Credit, and Banking* 19, no. 4 (November 1987): 457–68.
28. See n27 above, as well as O.M.W. Sprague, *History of Crises under the National Banking System* (Fairfield, N.J.: Augustus M. Kelley, 1977 [1910]).
29. Kevin Dowd, “Option Clauses and Bank Suspension,” *Cato Journal* 10, no. 3 (Winter 1991): 761–74.
30. White, 25–30.

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THE ROARING TWENTIES AND
THE BULLISH EIGHTIES:
THE ROLE OF GOVERNMENT
IN BOOM AND BUST

There are significant parallels between the Roaring Twenties and the Bullish Eighties. Both decades were characterized by a policy-induced artificial boom that ended with an inevitable bust. The Federal Reserve had a hand in both episodes, keeping the interest rate artificially low in the first one and keeping Treasury bills artificially risk-free in the second. Comparing the two episodes in terms of Federal Reserve policy, federal government borrowing, and the regulatory environment faced by the banking community accounts for both similarities and differences between the economic realities of the 1990s and those of the 1930s.

A good friend of mine has two sons who, in their youth, were unusually mischievous. On one occasion, when my friend had just replenished his liquor supply in preparation for a cocktail party that evening, his sons decided that a liquor cabinet was a pretty good substitute for a chemistry set. They broke the seals and poured from one bottle directly into the next: scotch into rum; rum into

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Roger W. Garrison, Department of Economics, Auburn University, Auburn AL 36849, telephone (205) 844-2920, telefax (205) 844-4016, wishes to thank James Barth and Sven Thommesen for their helpful comments. An earlier version of this paper was presented in May 1992 at a conference on the Federal Reserve sponsored by the Ludwig von Mises Institute of Auburn University held at Jekyll Island, Georgia (the cite of the signing of the Federal Reserve Act in 1913).

gin; gin into scotch. And they added a little crème de menthe all around. When their father discovered the deed (not in time to save the evening guests from some innovative cocktails), he issued punishment in the form of reduced allowances and increased yard duties. The two boys accepted the punishment gracefully and promised never to do that again. "You know," my friend told me, "I believe them. They'll never do *that* again. The next time, it'll be something else."

And so it is with the Federal Reserve. Mischievous by its very nature, it rarely does the very same thing twice. Fed watchers, always looking for a precise pattern in monetary aggregates, hoping to get an exact fix on the Federal Reserve's *modus operandi*, are almost sure to be disappointed. The enduring capacity of the Federal Reserve to exert a powerful influence on the course of economic events derives importantly from its adaptability. New trends in fiscal policy and modifications in the regulatory environment can change the nature and significance of Federal Reserve actions in ways that are difficult to perceive until after the fact.

In recent years, difficulties in perceiving just how the Federal Reserve is affecting the course of the economy have translated into doubts that the Federal Reserve has a significant effect—doubts even that money has much to do with the cyclical variation of employment and output. So-called real theories of the business cycle account for each departure from trend-line growth in terms of some real shock to the economy—which typically means a change in technology or in resource availability.¹ In turn, the focus on macroeconomically significant real shocks, which are relatively few and far between in comparison to monetary shocks, has caused many modern macroeconomists to believe that business cycles themselves are far less troubling than was once thought.² To similar effect, the increasing reliance on an analytical framework that reduces all macroeconomic phenomena to considerations of aggregate demand and aggregate supply has led textbook writers to emphasize the temporariness of cyclical variation rather than the pervasive discoordination and painful recovery that characterize boom and bust.³

Such treatments of cyclical variations and of the relationship between monetary and fiscal policy are fundamentally flawed. While important changes in the fiscal and institutional environment underlie the comparison between the Federal Reserve in the 1920s

and 1930s and the Federal Reserve in the 1980s and 1990s, the Federal Reserve's power to create money must figure importantly in the accounts of both periods. Understanding just how, though, requires an analysis that makes use of a level of aggregation much lower than that of conventional macroeconomics.

From Textbook Macroeconomics to Macroeconomic Realities

Macroeconomic policy is conventionally divided into two categories: monetary policy, which is formulated and implemented by the Federal Reserve, and fiscal policy, which is the net effect of the many spending and taxing decisions made by Congress. Macroeconomic textbooks typically introduce monetary and fiscal policies in separate chapters and then deal with the interplay between the two by constructing multiquadrant graphs in which the money supply, government spending, and the level of taxation, each represented in separate quadrants, have a combined economywide effect on the rate of interest and the level of income.

There is a certain logic to this policy decomposition. Inflating, spending, and taxing in the conventional macroeconomic framework have their own separate short-run effects on the interest rate and income level: expansionary monetary policy causes incomes to rise and interest rates to fall; expansionary fiscal policy (increased government spending or decreased taxation) causes both incomes and interest rates to rise. The effect of coordinated monetary and fiscal policy is simply the sum of the individual effects. Economic expansion driven by both the Federal Reserve and the federal budget, for instance, has a double-barreled effect on the level of income while leaving the rate of interest unchanged.⁴

Yet the relevance of such textbook treatments of policy hinges on several critical assumptions. By expanding the money supply, policymakers intend to affect output and employment rather than prices and wages. Any hopes for these intended real effects—as opposed to purely nominal effects—must be based on the assumption that prices and wages are somehow stuck above their market-clearing levels at the outset of the expansion and that the new money lent at lower interest rates is used only to mobilize otherwise idle resources. If, instead, pre-expansion prices and wages are fully adjusted to their market-clearing levels, then the effects of

monetary expansion are only temporary. In the long run, real incomes return to their pre-expansion levels as prices and wages adjust upward; real interest rates return to their pre-expansion levels as rising prices and wages build an inflationary premium into the structure of nominal interest rates. Similarly, expansionary fiscal policy, which increases real rates of interest, has only a temporary effect on incomes under conditions of flexible prices and wages. These assumptions and qualifications are acknowledged—though sometimes cryptically—in most modern macroeconomic textbooks.

But these treatments employ an exceedingly high level of aggregation, whereby “income” summarily measures both the total output produced in exchange for that income and the spending power capable of buying that output. This aggregation causes the phrase “temporary effects of fiscal and monetary policy” to seem innocuous or benign, seriously understating the actual effects of policy. The conventional wisdom is that policy in the form of such “stimulant packages” may temporarily push the activities of producing, earning, and spending beyond levels that can be sustained. At worst, the dynamics of policy-induced changes in macroeconomic magnitudes give scope for political chicanery as incumbent administrations resort to fiscal and monetary stimulants just prior to elections.⁵

According to an increasingly common view, however, cyclical movements in income and output—whether attributable to policy actions or to real factors—are considered harmful only in that the timing of consumption is slightly less than optimal. This assessment allows for a quantitative estimate of the welfare loss due to temporal suboptimality of approximately one-tenth of 1 percent of total consumption—which translates into about \$8.50 per person per year.⁶ Disaggregating the economy’s investment sector into policy-relevant patterns of investment, however, reveals that the temporary effects are not so benign. The scope for harm caused by monetary and fiscal stimulants can instead be seen in terms of unsustainable changes in the pattern of investment. Even if the spending power of income earners equals total output in aggregate terms, a systematic, policy-induced mismatch between decisions in the investment sector and the underlying preferences of consumers and wealth holders can lead to severe economic downturns and painful recoveries.

By carefully identifying the relevant aspects of investment patterns in different cyclical episodes, we can identify both theme and variation in the story of boom and bust. We can find both similarities and differences, for instance, in comparing the experience of the 1920s and 1930s with that of the 1980s and 1990s. Further, we can show that the prolonged succession of policy-induced “temporary” effects, which has fundamentally changed the relationship between fiscal and monetary policy, has had permanent effects on the health of the economy.

Variation on a Theme

How strong are the parallels between the boom of the 1920s and the boom of the 1980s? How similar are the economic circumstances of the early 1990s to those of the early 1930s?

It may be tempting to try to account for our current macroeconomic plight by retelling the story of the interwar experience, changing only the dates and a few minor details. But the story doesn’t fit that well. Credit conditions as judged by real rates of interest were relatively tight during the 1980s in comparison to credit conditions during the 1920s. And although the overall monetary expansion was actually greater in the more recent episode, the pattern of monetary growth in the two periods differs importantly. In the 1920s, the money growth rate peaked near the end of the decade as the Federal Reserve attempted with increasing resolve to keep the boom going; in the 1980s, the peak growth rate of M_1 came at mid-decade, after which monetary growth fell to low single digits while the bull market continued. Adjusting the story by replacing the conventional money or credit aggregates with more narrow ones, such as the monetary base, or with broader ones, such as the Divisia index, does little to improve the fit. And given the intense Fed watching in recent decades, it would in fact be surprising to learn that the Federal Reserve had nonetheless ignited and sustained an artificial boom for several years by simply repeating its misdeeds of the 1920s. There is, after all, a kernel of truth in the notion of “rational expectations”—as recognized by Ludwig von Mises years before that term achieved currency in macroeconomic thought.⁷

Parallels can be found not in the strict sense of a replay but in the

broader sense of variation on a theme. The story requires a recasting of the characters and some major changes in the plot. The Federal Reserve no longer plays the lead; it plays instead an indispensable supporting role. Banking legislation and fiscal policy are more central to the recent storyline. In accounts of both periods, however, we can say that unprecedented conditions allow an artificial boom to go unchecked for a significant period of time. Unprecedented in the 1920s was a strong central bank bent on stimulating growth in a peacetime economy. Unprecedented in the 1980s was a banking industry operating in a dramatically altered regulatory environment and a federal government running deficits measured in the hundreds of billions.

Interest rates in the recent episode play an important role not so much because of considerations of time discount but because of considerations of risk. During the 1920s, the low time discount, signaled by artificially depressed interest rates, did not accurately reflect people's actual willingness to save; during the 1980s, the low risk premiums built into interest rates did not accurately reflect people's actual willingness to accept the risks of increasingly speculative investments—much less the additional risks attributable to the government's irresponsible fiscal policy. The boom of the 1980s was no less artificial, however, than the one in the 1920s. To see why, we shall have to shift our focus from the easy money provided by the Federal Reserve in the 1920s to the risk-free securities provided by the Treasury in the 1980s. But first let us highlight aspects of the 1920s that have identifiable counterparts in the 1980s.

The Federal Reserve played a leading role in the dramatic boom of the 1920s (and the bust of the 1930s). Artificially cheap credit provided by the Federal Reserve underlay the economic expansion that lasted through mid-1929. This credit expansion, in an economic environment largely devoid of Fed watchers, drove a wedge between saving and investment. Guided by low rates of interest, investment outstripped saving in aggregate terms, and—more importantly—investment projects were excessively long-term. As the boom proceeded, low interest rates lured capital into relatively time-consuming production processes. That is, the timing of the output of these production processes was skewed toward the future in comparison to the intertemporal pattern of demand for output. While this intertemporal distortion of output is the essence of so-called real business cycle theory, it is only a symptom, in the

view presented here, of a pervasive distortion in the economy's capital structure. The economywide inconsistencies—attributable to Federal Reserve policy—between investment decisions of the business community and the time preferences of consumers made the bust inevitable. The recovery, hampered by policies aimed at reigniting the boom, consisted of extensive capital liquidation and a general intertemporal restructuring of capital.

Modern textbook treatments of the recent economic boom in comparison to the interwar boom hinge on a sharp distinction between monetary and fiscal policy. The earlier boom was driven by monetary policy; the later one by fiscal policy. It is true that the 1920s were characterized by (relatively) tight fiscal policy and loose monetary policy as each is conventionally measured, and that the 1980s saw a reversal in the relative strengths of the two policy alternatives. But the strict dichotomization between fiscal and monetary policy is badly overdrawn. In the 1980s, the significance of fiscal policy lay not in its augmentation of aggregate demand but in the private-sector risks and uncertainties that were attributable to chronic and dramatic federal budget deficits. This shift in focus directs attention to the Federal Reserve's critical supporting role throughout the decade and to the banking legislation at its beginning.

While irresponsible fiscal policy created additional risks and uncertainties to be borne by the private sector, the Federal Reserve in its capacity to monetize Treasury debt kept the risk premium off Treasury securities. Further, while extensive changes in the regulatory environment faced by the banking industry led banks to take on increasingly riskier portfolios, the Federal Reserve in its capacity of lender of last resort—together with policies of the FDIC—kept the risk premium off bank securities and minimized the worries of the banks' depositors. Although the story of the 1980s is institutionally complex, the general nature of the problems in the private sector is relatively simple. The regulatory and policy environment led the business community to take on risks that were systematically out of line with the risk preferences of private wealth holders. This systematic discrepancy between risks undertaken and risk preferences, which provides the thematic link to the interwar episode, justifies the claim that the 1980s boom was artificial and that the bust was inevitable.

Deficit-Induced Uncertainties

It is not difficult to demonstrate that chronic and dramatic federal budget deficits create uncertainties in the private sector.⁸ A numerical example can serve to illustrate. Suppose the government's anticipated rate of spending over the next several years is a trillion dollars per year, and that it anticipates collecting \$800 billion per year in tax revenues. The difference, the anticipated annual deficit, of \$200 billion represents yet-to-be-funded government spending.

The business community understands that the government will appropriate a trillion dollars worth of resources each year. Tax codes stipulate the particular targets of 80 percent of the government's appropriation activities. Production plans can be made in the light of these codified taxing procedures. But there can be no plans that effectively take into account the other 20 percent, the anticipated deficit. In effect, the government is putting the private sector on notice: "We are planning on appropriating another \$200 billion worth of resources, but we are not saying just how, just when, and just whose."

The government may continue issuing new Treasury bills while holding the line on the money supply. This would mean continued strains on credit markets, real rates of interest higher than they otherwise would be, and continued trade deficits as the Treasury sells those bills both at home and abroad. Alternatively, the government may rely more heavily on money creation. The Federal Reserve may begin buying Treasury bills at an accelerated rate. This process of debt monetization would take the pressure off credit markets and strengthen export markets. It would reduce the real rate of interest (temporarily) but would build an inflation premium into the entire structure of interest rates. As still another alternative, the government may institute new taxes of some kind or raise tax rates in some yet-to-be-specified way. In the meantime, a \$200-billion cloud of "intent to appropriate in some unspecified way" looms large over the private sector.

There is no effective hedge against uncertainty of this kind. There are no probabilistic answers to the question of just how the government will appropriate the additional resources. Should long-term capital be shifted out of export industries because of the currently high foreign-trade deficit and correspondingly weak export markets? Or should it be kept in place by anticipations of—or

hopes for—a change in fiscal strategy? Should long-term financial commitments be based on current credit conditions or on the contingency of some unknown likelihood that the Treasury will borrow more heavily in domestic as opposed to foreign credit markets? Should land, durable assets, and even inventories be bought or sold at prices that reflect the current inflation rate? Or should such transactions reflect accelerating inflation based on some guess about the extent and timing of debt monetization?

Although the government's borrowing at irresponsibly high levels adds to the riskiness of private-sector activities, none of these risks is borne by the holders of Treasury securities. This discrepancy between risk created and risk assumed can be directly attributed to the Federal Reserve in its capacity to monetize Treasury debt. Overextended borrowers in the private sector must pay a substantial default-risk premium in order to continue borrowing. Even overextended municipalities pay a default-risk premium as their bonds are downgraded by bond-rating agencies. The power to tax alone is not enough to protect municipal bondholders against default. But the interest rate paid by the federal treasury contains no default-risk premium at all. The Federal Reserve stands ready to monetize the Treasury's debt in circumstances that otherwise would require an outright default. It is true, of course, that *actual* monetization imposes costs in the form of price distortions and a general price inflation, but these costs are imposed on the economy in general—not just the holders of Treasury securities. Since a "monetization risk," unlike a default risk, is borne by holders and nonholders alike, there is no monetization-risk premium—separate from the economywide inflation premium—built into the nominal yield on Treasury securities. The very potential for debt monetization is what breaks the link between fiscal irresponsibility and some corresponding risk premium.

The Federal Reserve, then, plays a critical supporting role in the pursuance of fiscal policy. Relieving the holders of Treasury securities of any risk burden increases the attractiveness of those securities and thus eliminates what would otherwise be a binding market constraint on further Treasury issues. The increasing significance of *potential* debt monetization suggests that the magnitude of the Federal Reserve's influence is not to be detected in *actual* movements of monetary aggregates. The mere fact that the Federal Reserve stands

ready to monetize debt gives the Treasury a much longer leash than it would otherwise have.

The Artificial Boom

Textbook treatments of fiscal and monetary policy recognize that the fiscal authority and the Federal Reserve can work together. The Treasury issues debt and the Federal Reserve monetizes it. So long as government borrowing has not been pushed to irresponsible levels, debt issue and monetization have short-run effects on output and incomes that reinforce one another and short-run effects on the interest rate that cancel one another. These effects of policy are derived straightforwardly from standard analysis which focuses on aggregate supply and aggregate demand. But when borrowing becomes excessive, considerations of risk become dominant. Going beyond the circumscribed focus of the textbook, we can recognize that the Treasury creates risk and the Federal Reserve externalizes it.

To say that the Federal Reserve keeps the default-risk premium off Treasury bills is not to say that the risk is actually eliminated. The burden of bearing it is simply shifted from the holders of Treasury securities to others. Borrowing and investing in the private sector becomes more risky than it otherwise would be. Holders of private debt and equity shares must concern themselves not only with all the usual risks and uncertainties of the marketplace but also with the risks and uncertainties attributable to changes in the way the federal deficit is accommodated. Selling Treasury bills in foreign credit markets, in domestic credit markets, or to the Federal Reserve can have major effects on the strength of export markets, on domestic interest rates, and on the inflation rate. The inability of market participants to anticipate the Treasury's borrowing strategy translates into unanticipated changes in the value of private securities.

If the additional risks attributed to federal budget deficits and imposed on the private sector were allocated in some economically efficient way, there would have been no artificial boom arising from the irresponsible fiscal policy of the 1980s. The willingness to lend and to buy equity shares in the private sector would have been generally reduced, as wealth holders opted for the artificial security

provided by government debt; but the reduction in private-sector activity would have been minimized so long as the additional risks were assumed by those most willing to do so. This result, though, was precluded by institutional factors that hid the private-sector riskiness from those who were (unknowingly) financing risky undertakings. Again, the Federal Reserve plays a strong supporting role, as does the Federal Deposit Insurance Corporation. Together, they enabled commercial banks and their depositors to finance risky ventures throughout the 1980s while being shielded either permanently or temporarily from the risks. This shield from risk bearing, like the low rate of interest in the 1920s, gave rise to an artificial boom and subsequent bust.

The Depository Institutions Deregulation and Monetary Control Act of 1980 (DIDMCA) dramatically changed the banking industry's ability and willingness to finance risky undertakings. Increased competition from nonbank financial institutions drove commercial banks to alter their lending policy so as to accept greater risks in order to achieve higher yields. The Federal Reserve in its long-established capacity of lender of last resort diminished the banks' concerns about possible problems of illiquidity while the FDIC absolved the banks' depositors of all worries about illiquidity and even about bankruptcy. Riskier loans, then, were only partially reflected in higher borrowing costs and lower share prices. In substantial measure, specific private-sector risks were transformed by DIDMCA, the Federal Reserve, and the FDIC into (1) the generalized risk of inflation in the event of excessive last-resort lending by the Federal Reserve, and (2) the risk of a large and unbudgeted liability in the event of excessive last-resort closings by the FDIC. Thus, economic activity in the private sector was spurred on by the lure of higher yields, yet it was largely unattenuated by considerations of risk, which were effectively externalized and diffused.

The artificially low risk premiums stemming from the risk-externalizing effect of potential debt monetization in the 1980s paralleled the artificially low interest rates created by actual monetary expansion in the 1920s. What was without an earlier parallel, however, was the impact of deposit insurance in the post-DIDMCA period.⁹ Throughout the 1980s, the FDIC continued to protect depositors while charging the banks a premium that was too low in general and, more significantly, that was unrelated to the riskiness of bank assets. This subsidy to risk taking may have been signifi-

cant enough, by itself, to create an artificial boom. There was no difficulty in finding risks to take. Banks could simply lend more heavily to overextended farmers, third-world countries, oil prospectors, and real-estate developers; or they could find new risks such as those created by leveraged buyouts and the dramatic growth of the junk-bond market. It was the financial sector's demand for high-risk, high-yield securities, in fact, that gave junk bonds and other highly leveraged securities their buoyancy.

Although it is possible to think of the FDIC as having its own independent effect throughout the 1980s, FDIC policy was actually an integral part of the fiscal, monetary, and regulatory environment that created and externalized risks. The Treasury created risk; the Federal Reserve and the FDIC externalized it. After all, speculative lending such as for commercial real-estate development or for highly leveraged financial reorganizations are risky in large part because of possible changes in such things as the inflation rate, interest rate, trade flows, and tax rates—the very things that can undergo substantial and unpredictable change when the federal budget is dramatically out of balance. The 1980s may best be understood, then, as a decade in which the policy-induced externalization of risk gave rise to a substantial but ultimately unsustainable economic boom.

The Bust

Potential debt monetization can keep Treasury bills risk-free for the indefinite future; the reimbursement of depositors of failed banks can continue so long as the FDIC can be recapitalized out of general tax revenues. But the banking industry cannot be shielded from the consequences of excessive risk taking forever. For almost a decade the banking industry and the speculative activities it supported were able to keep the economic expansion going. Although risk aversion normally characterizes sound banking, high-flying banks in the 1980s were able to indulge in risky lending despite the preferences of their depositors and to escape both market-imposed or government-imposed discipline until the cumulative effects of externalizing risk turned the undue risk taking into a financial crisis. The Federal Reserve's routine functioning as lender of last resort, the FDIC's *de facto* policy of forbearance in cases of problem

banks, and the implicit acceptance of the doctrine of “too big to fail,” all help to account for the length of the artificial boom. But neither increased last-resort lending and forbearing nor more overt inflationary finance, such as was pursued in the 1920s, could keep the boom going indefinitely. As with the artificial boom in the interwar period, an eventual bust was inevitable.

Like the time-consuming production processes that were out of line with time preferences, speculative loan portfolios that were out of line with risk preferences generated an artificial boom in the 1980s that belonged the same general class as that of the 1920s. However, the distinction between economic activities that are excessively future oriented and economic activities that are excessively speculative—together with some institutional considerations—allows us to see systematic differences between the 1930s and the 1990s.

First, the downturn at the end of the Bullish Eighties came in the form of a bank-led bust. A high rate of bank failures was experienced well before the general economic contraction. At the end of the Roaring Twenties, by contrast, the bank failures came after the economic contraction had begun. This difference in the timing of events is consistent with differences in the nature of the two expansions. Industrial borrowers in the 1920s were using newly created funds for excessively capital-intensive ventures that, in general, were not otherwise excessively speculative. It is true, of course, that there was heavy speculation in securities markets in the 1920s—much more so then than in the 1980s—but the cause-and-effect relationship in the recent episode was the reverse of that in the earlier one. That is, in the 1920s, monetary expansion, which allowed banks to support heavy industry, also fueled speculation in securities markets. However, because the risks of that speculation were borne, in the first instance, by the buyers of the securities, there was no policy-induced externalization of risk to weaken banks even as the expansion continued. In the 1980s, policy-induced speculation, on the part of the banks themselves and their industrial borrowers, eroded bank capital, weakening the banks throughout the boom—so much so that the erosion of their capital base eventually turned boom into bust.

Second, while the idleness of plant, equipment, labor, and other resources that characterized the 1930s has its counterpart in the semi-idleness in the 1990s, the disposition of unprofitable assets is

different now, largely because the recent bust was bank led. During the Great Depression, firms whose revenues did not cover operating costs simply closed their doors. Work on incomplete industrial projects whose present value had turned negative was simply discontinued. Although this form of market discipline was sometimes delayed by policies aimed at rekindling the boom, eventually resource idleness characterized those sectors of the economy that were most out of line with underlying economic realities, and liquidation could proceed apace.¹⁰ In the current slowdown, many failing firms are first identified as nonperforming loans in the portfolios of failed banks. As insolvent banks are closed by the FDIC, the bad loans are transferred to the Resolution Trust Corporation (RTC), which functions as a caretaker until it can sell the assets. In many cases, the physical assets, such as franchised motels or restaurants, are not idled. Instead, the RTC contracts with an operating company to run the business. The contract allows the operating company to earn a profit while minimizing the cost to the RTC of maintaining the assets.

The existence of many such failed-but-still-operating businesses, including firms undergoing bankruptcy proceedings but still operating with the newly evolved debtor-in-possession (DIP) financing, helps to explain why the current recession is a relatively shallow one by conventional measures. What otherwise would be idle capital is partially masked by RTC policy as underemployed capital—analogous to the underemployed labor associated with 1930s-style make-work projects. “Zombie banks,” banks that are allowed to continue operations after their net worth has turned negative, have their counterpart in RTC-owned or DIP-financed “zombie firms.”¹¹

While the underemployed capital in zombie firms limited the depth of the recession, it added to the length. Recovery consists of re-employing resources idled by the bust. As confirmed by experience in the early 1990s, it would have been easier to draw resources out of idleness than to draw them away from the RTC. Asset managers of the RTC, trying to avoid the ruination of markets that dumping real assets at fire-sale prices would entail, stockpiled them instead, creating a huge “overhang” which added significantly to the uncertainties in the private sector. Also, solvent firms and would-be upstarts, which would have to raise their own capital to expand or enter the market, are not eager to compete with bank-

rupt firms or with privately operated but RTC-owned businesses whose revenues do not have to cover the costs of capital. Considerations of these sorts help to explain why the government's recent recourse to monetary stimulation in the form of exceedingly low discount rates has met with such little success.

Third, the unemployment currently being experienced has a distinctly different composition from that of the 1930s. It is widely reported that white-collar workers are disproportionately affected in the current recession as compared to earlier cyclical downturns. The time-preference/risk-preference frame of analysis makes this composition difference readily understandable. The boom in the 1920s involved resources allocated disproportionately to capital-intensive projects, such as steel mills and manufacturing plants. The labor complement to heavy industry tends to be predominantly blue-collar. The boom in the 1980s involved resources allocated to speculative development, such as commercial real estate and financial services. The labor complement to this kind of capital tends to be predominantly white-collar. In both episodes, the composition of unemployment matches the pattern of capital misallocation.

Finally, macroeconomic policy after the bust reveals a critical difference between the current situation and that of the 1930s. When further monetary expansion, which sustained the boom of the 1920s for nearly a decade, could sustain it no longer, both the monetary expansion and the boom came to an end. The public's increased demand for currency relative to checking-account money, coupled with the greater reluctance to lend on the part of commercial banks, swamped the Federal Reserve's efforts to reflate.¹² Despite the further padding of the monetary base, the dynamics of the bust itself was an effective check against continued monetary expansion. By contrast, when further deficit spending and risk externalization, which sustained the boom of the 1980s, could sustain it no longer, the boom ended, but the deficit spending and risk externalization escalated. In fact, lower tax revenues and higher payments of entitlements, both associated with the recession, led to still more government borrowing. The dynamics of the bust, then, provided increased scope for the very kind of irresponsible fiscal policy that made the bust inevitable.

How Little "We" Know

The failure at the dawn of the last decade to extend deregulation to the provision of deposit insurance, and the absence of any market check against the Treasury's fiscal excesses, provide dramatic illustrations of the general fallacy of the mixed economy. Privatized profit seeking coupled with socialized risk bearing undergirded both the bull market of the 1980s and the harsher economic realities of the 1990s. The risks assumed by lenders and borrowers, savers and investors, hedgers and leveragers are rendered inconsistent with the actual risk preferences of wealth holders in the marketplace by the FDIC subsidy to risk bearing and by the Fed-backed Treasury, whose power to issue risk-free debt imposes risks on the private sector.

Researchers at the Federal Reserve are just two steps away from recognizing the problem of deficit-induced uncertainties, as evidenced by a recent article entitled "How Little We Know about Deficit Policy Effects."¹³ Macroeconomic data as illuminated by several sophisticated modeling and econometric techniques have led two economists at the Minneapolis Federal Reserve Bank to conclude with confidence that "deficit policies may matter, and then again they may not. Existing studies really don't tell us much about their effects."¹⁴ The first step from this disturbingly limp conclusion to a healthy understanding of the deficit problem is to recognize that the "We" in the title of the article, intended to mean "We Economists," can be extended to mean "We Lenders-Borrowers-Savers-Investors-Hedgers-Leveragers" or simply "We Market Participants." Market participants do not know how deficit accommodation will affect future market conditions, so they have to make guesses. And if they guess wrong, they may lose big. The second step is to recognize that the "We" may also refer to the holders of Treasury securities. Accordingly, the title phrase should be amended to read "How Little We Know *or Care* about Deficit Policy Effects." The potential for debt monetization, as manifested by the Federal Reserve in its stand-by capacity, has absolved the Treasury's creditors of any inclination to care. Externalizing risk has precluded any possibility that the reluctance of creditors will provide an effective check against the excesses of the Treasury.

The tripling of federal government indebtedness since the beginning of the 1980s bull market stands as testimony to the capacity of

the Treasury to issue its artificially risk-free debt. The banking legislation of 1980 has shown us its capacity for blinding the banking industry and the private sector to the black cloud of debt gathering above it. Together, the actions of the fiscal and monetary authorities have demonstrated once again how public institutions ostensibly devoted to stability and prosperity are, in the end, responsible for crises and decay.

NOTES

1. Real business cycle theorists take the empirically demonstrated links between money and economic activity to be a result of "reverse causation": changes in the money supply are seen as the effect rather than the cause of changes in economic activity. For a critical account of this and other aspects of real business cycle theory, see Mark Rush, "Real Business Cycles," *Federal Reserve Bank of Kansas City Economic Review* 72, no. 2 (February 1987): 20–32.
2. "Real cycle theorists question . . . the conventional wisdom, which asserts that business cycles harm the economy." *Ibid.*, 26. Rather than causing the economy harm, minor changes in the economy's output are understood to be a consequence of an efficient market coping with minor (macroeconomically speaking) changes in underlying realities. (In this context, the Great Depression is seen as something of an "outlier" not well accounted for by this or any other theory of the business cycle.)
3. Long-run aggregate supply, which reflects resource availabilities and technology, is invariant with respect to the price level and hence is represented by a vertical supply curve. Thus, the real effects of a money-induced change in aggregate demand, as would be measured horizontally, are strictly temporary, lasting only until the price level has become adjusted to the larger money supply.
4. A typical textbook treatment of macroeconomic policy is contained in William J. Baumol and Alan S. Blinder, *Economics: Principles and Policy*, 5th ed. (New York: Harcourt Brace Jovanovich Publishers, 1991), chs. 11, 13, and *passim*.
5. For early and nontrivial formulation and application of modern political business cycle theory, see William D. Nordhaus, "The Political Business Cycle," *Review of Economic Studies* 42, no. 130 (April 1975): 169–90; and Edward R. Tufte, *Political Control of the Economy* (Princeton: Princeton University Press, 1978).
6. Robert E. Lucas, Jr., *Models of Business Cycles* (London: Basil Blackwell, 1987), 27. Despite his low estimate of the social cost of cyclical variation of output, Lucas rejects real business cycle theory on the grounds that the candidates for real shocks are too small to account for actual fluctuations.

Ibid., 71. Textbook authors typically offer no quantitative estimate of the harm attributable to business cycles, but the fact that Baumol and Blinder's 900-page textbook devotes less than six pages—and nowhere more than two consecutive pages—to the subject of business cycles carries its own message.

7. Ludwig von Mises, *The Theory of Money and Credit* (New Haven: Yale University Press, 1953; originally published in German in 1912). Mises fully recognized the strategic significance of expectations in his 1953 addendum entitled "Monetary Reconstruction." He expressed the limits to—and the short-run nature of—inflationary finance in terms of Lincoln's Law: you can't fool all the people all the time (see 419).
8. Deficit-induced uncertainties underlie the arguments in Roger W. Garrison, "Public-Sector Deficits and Private-Sector Performance," in Lawrence H. White, ed., *The Crisis in American Banking* (New York: New York University Press, 1992), 29-54.
9. For a treatment of the effects of FDIC policy, see Roger W. Garrison, Eugenie D. Short, and Gerald P. O'Driscoll, Jr., "Financial Stability and FDIC Insurance," in Catherine England and Thomas Huertas, eds., *The Financial Services Revolution: Policy Directions for the Future* (Boston: Kluwer Academic Publishers, 1987), 187-207.
10. On the role of government in adding to the severity of the Great Depression and delaying recovery, see Gene Smiley, "Can Keynesianism Explain the 1930s?: Reply to Cowen," *CRITICAL REVIEW* 5, no. 1 (Winter 1991): 81-114; and Richard K. Vedder and Lowell E. Gallaway, *Out of Work* (New York: Holmes and Meier, 1993), 74-149.
11. The term "zombie S & Ls" was coined by Edward J. Kane in the context of the savings-and-loan crisis, which was a precursor to the crisis in the banking industry and subsequent recession. See Edward J. Kane, "Dangers of Capital Forbearance: The Case of the FSLIC and the 'Zombie' S & Ls," *Contemporary Policy Issues* 5, no. 1 (January 1987): 77-83. For a healthy perspective on RTC policy and DIP financing, see Stephen Delos Wilson, *The Bankruptcy of America* (Germantown, Tenn.: Ridge Mills Press, 1992), 81-96 and *passim*.
12. See Smiley, 88.
13. Preston J. Miller and William Roberds, "How Little We Know about Deficit Policy Effects," *The Federal Reserve Bank of Minneapolis Quarterly Review* 16, no. 1 (Winter 1992): 1-12.
14. Ibid., 8.

THE RECESSION AND AUSTRIAN
BUSINESS CYCLE THEORY:
AN EMPIRICAL PERSPECTIVE

How well is Austrian business cycle theory corroborated by empirical evidence? This question is addressed by examining the contraction of 1990–1991 and the expansion leading up to it. An overview of the Austrian theory of the business cycle permits the identification of several empirical propositions implied by the theory. Empirical data for several economic variables are examined for consistency with the patterns suggested by the theory. The evidence suggests a muted Austrian cyclical process at work in conjunction with other factors during the time period studied.

Compared with previous post–World War II recessions, the recession of 1990–1991 was not especially virulent.¹ The standard array of macroeconomic data suggests a downturn substantially less severe than advertised.² Yet the recovery has, by historical standards, been sluggish. Thus, even though the recent recession’s unemployment rate peaked at just below the post–World War II recession average (7.7 percent vs. 7.8 percent), that peak occurred 15 months after the recession’s trough, compared to a post-trough average of 3 months for other post–World War II recessions.³

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William N. Butos, Department of Economics, Trinity College, Hartford, CT 06106, telephone (203) 297–2448, telefax (203) 297–2257, thanks Steven Horwitz for suggestions in preparing this paper, three anonymous referees for their thoughtful comments, and especially Jeffrey Friedman for his invaluable assistance in bringing this paper to publication. All remaining errors are the author’s.

The atypical pattern of recovery has so dominated public discourse that little attention has been given to the causes and character of the recession itself. The aim of this paper is to provide an overview of the expansion during the 1980s and the downturn that followed. The major findings are that despite a generally declining rate of inflation since 1982 (which might otherwise suggest monetary policy restraint), federal policy actually engendered an unsustainable expansion that required an economywide correction. This garden-variety "what goes up must come down" story, however, becomes complicated by a rather unusual confluence of other forces that affected the economy during the 1980s.

The Federal Reserve stimulated an economic expansion that began late in 1982. The combination of changes contained in the Tax Reform Act of 1986 and the Federal Reserve's move toward monetary restraint beginning in the second half of 1986 (and continuing through the end of the decade) brought the expansion to an end in certain sectors of the economy. These corrections presaged the general contraction at the end of the decade. The prerecession and sectoral character of these adjustments meant that the expansion that began in November of 1982 required only a modest correction (by historical standards) during 1990-1991. Had the Federal Reserve continued fostering easy money throughout the second half of the decade, and if the massive readjustments in the real-estate market induced by changes in the federal tax code not taken place, the eventual downturn would have been significantly more pronounced.⁴

The theoretical perspective employed here draws on the Wicksell-Mises-Hayek theory of industrial fluctuations, what is ordinarily called the Austrian theory of the business cycle (ATBC). In the next section the Austrian approach will be briefly overviewed for its major theoretical insights and for the kinds of empirical patterns in the data that would be consistent with it. Then I focus on the 1982-1989 expansion and the 1990-1991 recession. The empirical record reveals patterns generally (but not decisively) consistent with those suggested by the ATBC. The last section summarizes the main findings of the paper.

The Austrian Theory of the Business Cycle

The ATBC derives from Knut Wicksell's monetary theory and Austrian capital theory.⁵ Wicksell's contribution was to analyze the "cumulative process" associated with a divergence between the market rate and the "natural" rate of interest. The market rate of interest is the prevailing, observable rate that equilibrates the supply and demand for loanable funds. Wicksell's "natural rate" is the rate consistent with full economic equilibrium in the sense of equality of ex ante (i.e., planned) voluntary saving and investment flows. It is also the rate that would be equal to the generalized rate of profit and the rate of return on the marginal unit of capital.⁶ Wicksell claimed that when the market and natural rates were equal, the economy would be in equilibrium and that the purchasing power of money would be constant.⁷

Wicksell's analysis indicates that if the natural rate is above the market rate, prices will rise. This "cumulative (price) process" occurs because the money stock must be continuously increased in order to keep the market interest rate below its equilibrium level. The disequilibrium brought on by the divergence in interest rates results in a cumulative inflation (if the market rate is kept below the natural rate) or a cumulative deflation (if the market rate is kept above the natural rate).⁸ Equilibrium is restored only when the market rate of interest is allowed to reach the level of the natural rate of interest. But the conditions consistent with monetary equilibrium per se were not primarily what intrigued Wicksell and his followers; rather, their central interest was the economics of monetary disequilibrium. Wicksell's framework invites an analysis of what goes on when the two interest rates diverge and what happens to bring them back to equality, that is, the analysis of the path of the economy as a sequence of adjustments, what Ludwig von Mises called "step-by-step" analysis or what contemporary economists might refer to as "process analysis."

Wicksell's model was essentially a price-adjustment one; output was assumed to be fixed at full employment. Early on, however, Mises⁹ observed that a divergence between the market and natural rates of interest also necessarily affects the size and composition of output and relative prices in systematic ways. As initially developed by Mises and later by F. A. Hayek, the Wicksellian framework was augmented with Eugen von Böhm-Bawerk's theory of

capital in order to analyze the effects on the "time allocation" of resources arising from a divergence in the two rates of interest. The resulting framework thus attempts to analyze business cycles by focusing on market distortions that primarily affect intertemporal resource allocation.)¹⁰

The ATBC attributes the initiation of a business cycle to a central bank-induced expansion in credit that depresses the market rate below the natural rate of interest.¹¹ Austrian cycle theory is meant to explain business cycles of "intermediate" duration, ordinarily understood to involve a period of about 5-10 years, in contrast, for example, with "short" 1-3 year inventory cycles and to "long" growth cycles.¹² The central claim of the ATBC, as Fritz Machlup observed, is that "*monetary* factors *cause* the cycle but *real* phenomena *constitute* it."¹³ According to the ATBC, the expansion of bank credit in the commercial loan market, by cheapening the cost of capital, redirects the allocation of resources toward more capital-intensive (or "roundabout") methods of production. By lowering the market rate of interest, the central bank increases the rate of profit in the "higher stages" of production relative to those "lower stages" that produce consumer goods. Thus, the expansion will involve the allocation of human and material factors of production away from less roundabout uses and toward more roundabout investments made profitable by the expansion of credit. According to the ATBC, this pattern of reallocation will raise the prices of capital goods relative to the prices of consumer goods.

The larger and longer the credit expansion persists, according to the ATBC, the more roundabout or "top heavy" the structural organization of production should become. Had these changes in the structure of production been induced by increases in the supply of voluntary savings, the fall in the market rate of interest would be fully compatible with the preferences of individuals. The decline in the market rate would then parallel the fall in the natural rate of interest, and the resulting reconfiguration in the pattern of resource allocation would represent a sustainable equilibrium. But if the market rate of interest is artificially lowered by credit expansion, the reallocation of resources thereby induced is not compatible with the underlying preferences of individuals. In effect, the quantity of capital individuals choose to make available for investment through their savings is insufficient to maintain the top-heavy capital structure. Accordingly, a corrective adjustment process must

eventually restore the economy to equilibrium. This corrective process—the recession phase of the business cycle—liquidates those investments that were made possible by the preceding credit expansion; it will be more or less severe depending on the magnitude of that expansion. The expansion comes to an end when commodity prices rise relative to producer good prices, signifying an increase in the profitability of less capital-intensive methods of production. During the downswing, both people and nonhuman resources become unemployed as firms liquidate unprofitable ventures, downsize, or go out of business. Once an unsustainable expansion of credit has occurred, a recession is necessary to move the economy back toward less roundabout methods of production, allowing human and retrievable nonhuman factors of production to be reabsorbed into a sustainable pattern of activity.¹⁴

The ATBC gained wide popularity during the interwar years among academic economists, especially following Hayek's University of London lectures early in 1931 (which were published the following September as *Prices and Production*). But the chronic nature of the Great Depression, especially in America, not only raised doubts about the ATBC but generated support for policy measures that were directly opposed to those implied by the theory. The final blow against the ATBC was delivered in 1936 with Keynes's publication of *The General Theory*, which captured the imagination (even fervor) of most economists. With the flood tide of Keynesianism after World War II, the ATBC sank into obscurity, gaining only a small measure of respectability with the Austrian revival that began during the 1970s.¹⁵

With significant exceptions,¹⁶ the ATBC has infrequently been used to analyze actual cyclical episodes. But since it was its perceived irrelevance in accounting for real-world phenomena that doomed it to a half-century of neglect, examinations of empirical data seem to be called for if the theory is to warrant revival.¹⁷

The Recession of 1990–1991

The 1990–1991 recession was preceded by one of the longest expansions on record. According to the NBER cycle chronology, the expansion that began late in 1982 lasted until mid-1990, a span of 104 months. During this interval, the average annual increase in real

(inflation-adjusted) GDP was about 3.6 percent, while the rate of inflation (measured by the year-to-year change in the implicit GDP deflator) plunged from 10 percent in 1981 to 4.3 percent in 1990. Also, the civilian unemployment rate fell from 9.7 percent in 1982 to 5.5 percent in 1990.¹⁸

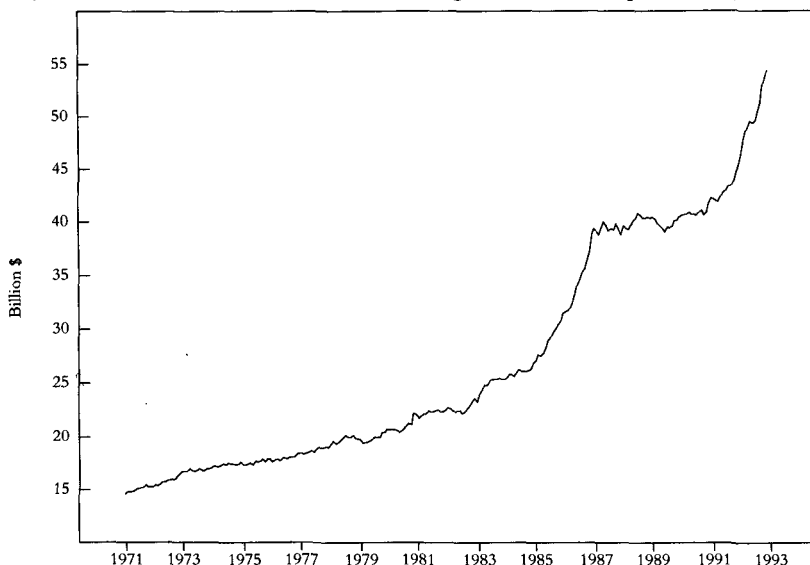
The ATBC, however, bids us to look more closely at several crucial indicators. Specifically, increases in bank credit and reductions in real interest rates¹⁹ are necessary conditions for the kind of expansion described by the ATBC. During the expansion, the ratio of producer goods prices to consumer goods prices should increase to reflect the increasing roundaboutness of production, and employment in capital-goods producing industries should rise. The initiation of a recession, according to the ATBC, should be signaled by an increase in the ratio of consumer goods to producer goods prices.²⁰ As the recession gathers steam, the ATBC holds that the pattern of economic activity should involve not only higher unemployment rates, but rising interest rates and a disproportionate contraction in the production of capital goods relative to consumer goods.

Credit Expansion in the 1980s

If monetary conditions are to set the stage for an Austrian cyclical process, bank loans must increase. Driving this increase is an excess quantity of bank reserves that enables lenders to increase their loans and lower the lending rate. In general, banks can only sustain such activities if the central bank increases the quantity of bank reserves.

The actual course of American monetary policy (in contrast to the Federal Reserve's intentions) may be seen by examining the quantity of reserves supplied by the Fed to the banking system. Bank reserves constitute the initial fuel enabling banks to make loans and create liabilities, thus affecting the pace of economic activity. Figure 1²¹ shows the path of total reserves from the early 1970s through early 1992.²² Until 1983, the average annual rate of growth in reserves was about 6.1 percent. From 1983 to 1987, however, reserves grew by an average rate of nearly 14.5 percent each year, indicating that monetary policy turned increasingly more expansive during the middle part of the decade.²³ (Interestingly, as

Figure 1: Total Reserves (adjusted for changes in reserve requirements)



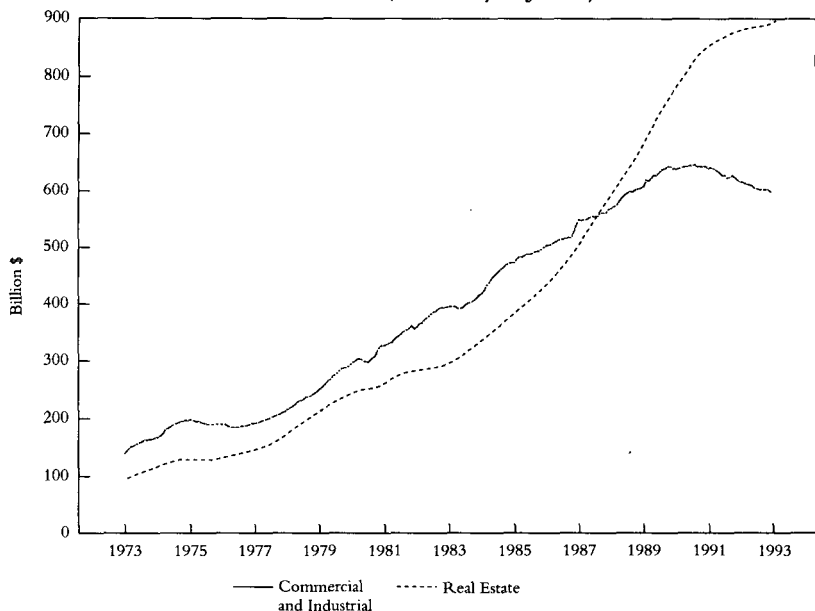
Source: Federal Reserve Board of Governors.

Figure 1 shows, the growth in reserves was virtually flat between 1987 and 1989 but has increased greatly since 1990.)²⁴

When we look to bank credit, however, the data do not entirely seem to meet the conditions specified by the ATBC. As Figure 2 shows, commercial bank's industrial and commercial loans increased steadily until the end of 1989. Although these loans fall off rather sharply beginning in 1990,²⁵ which is consistent with the ATBC, until that time they failed to expand as vigorously as might be expected given the increase in bank reserves.

Several factors may explain this result. First, the rapid increase in bank reserves relative to bank credit reflects the Fed's apparent desire to offset the strong decline in the income velocity of money, defined as the ratio of nominal GDP to the stock of money, which occurred between the end of 1983 and the end of 1986. Income velocity will decline if individuals increase their holdings of money relative to their incomes. This presumably is what happened as falling interest and inflation rates reduced the opportunity cost of holding money, and as an expanding array of interest-earning money assets became more widely available following the 1980 Depository Institutions Deregulation Monetary Control Act

Figure 2: Commercial Bank Loans (seasonally adjusted)



Source: Federal Reserve Board of Governors.

(DIDMCA) and the 1982 the Garn-St Germain Depository Institutions Act.²⁶

Second, the third-world debt crisis induced banks with third-world loans to substantially increase their loan loss reserves—reserves set aside (not loaned out) to cover nonperforming loans. By 1992 approximately 50 percent of the \$100 billion of debt held by U.S. banks was covered by loan loss reserves.

A third factor concerns the dramatic developments in the real-estate market during the 1980s. The data suggest that a significant element of the 1982–1990 expansion was real estate related. As Table 1 shows, in the 1980s commercial banks increased their real-estate loans at more than double the average rate of the other cyclical upswings. An important factor in accounting for the growth in real-estate loans was the 1981 Economic Recovery Tax Act (ERTA). ERTA provided substantially more favorable tax treatment of investment by accelerating depreciation schedules (15 years under ERTA compared to 40 years under pre-ERTA); reducing the asset lives of plant, equipment, commercial buildings, and rental housing; and

Table 1: Commercial and Industrial Loans & Real Estate Loans*
All Commercial Banks (seasonally adjusted)

	Annualized Real Percentage Change	
	Comm'l. & Indust.	Real Estate
NBER Expansions		
Nov 1970–Nov 1973	16.0	5.3
Mar 1975–Jan 1980	1.8	8.9
July 1980–July 1981	7.3	8.3
Nov 1982–July 1990	4.7	17.0
Average	7.5	10.0
NBER Contractions		
Nov 1973–Mar 1975	3.5	–.8
Jan 1980–July 1980	–8.5	–.5
July 1981–Nov 1982	6.8	3.2
July 1990–Mar 1991	–7.3	3.2
Average	–5.5	1.3

*nominal values deflated by CPI (for commodities)

Sources: Federal Reserve Board of Governors and Bureau of Labor Statistics.

providing a more generous investment tax credit on equipment (but not structures). These provisions had the somewhat unintended result of greatly increasing the attractiveness of real estate relative to other kinds of investment. The net effect of ERTA's tax sheltering advantages for real-estate investment was to generate enormous activity in commercial real estate.²⁷

Interest Rates in the 1980s

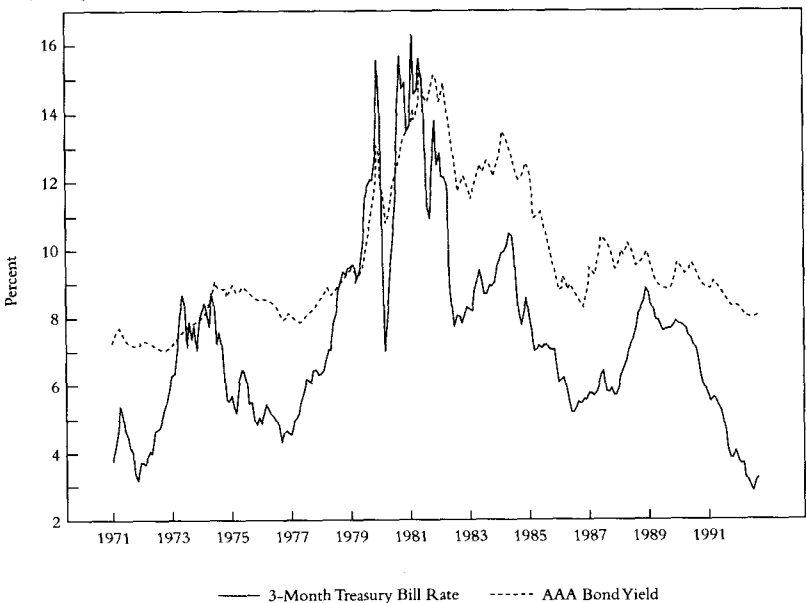
According to the ATBC, a business cycle expansion should be accompanied by falling real (i.e., inflation-adjusted) interest rates. The decline in real interest rates induces the capital-intensive expansion in economic activity associated with a cyclical upswing. This upswing comes to an end when the excessive expansion of bank credit ceases, creating a shortage of investable funds and causing interest rates to rise as firms attempt to effect their investment plans despite the lack of sufficient bank credit. The ATBC holds that real interest rates during the ensuing contraction should be above their levels during the expansion.

Figure 3 reveals a downward trend in both short- and long-term

rates during the 1980s. Nominal rates fell sharply as inflation was wrung from the economic system in the wake of the recession of 1980-1982. Ex post (i.e., realized) real interest rates climbed steadily from occasionally negative values in the 1970s and peaked in 1983-1984.²⁸ Thereafter, nominal rates continued a general trend downward, although from 1987 to 1989 short-term rates increased sharply. After 1989 short-term rates in both nominal and real terms fell again. Currently short-term rates are at very low or even negative levels after adjusting for inflation. The pattern of movements in long-term rates is somewhat different, peaking in 1982-1984 and falling somewhat steadily thereafter.

The fall in nominal and real rates during the expansion of 1982-1990 seems to have been brought about by expansionary monetary policy. Monetary ease is suggested by the growth in total reserves (as noted above) and by a 13 percent annualized rate of nominal M2 growth between 1982 and 1989.²⁹ Nevertheless, inflation rates fell and inflationary expectations seem to have eroded significantly throughout the decade. High money growth and re-

Figure 3: Nominal Interest Rates



Source: Federal Reserve Board of Governors; Moody's; Bureau of Economic Analysis.

duced inflationary expectations both act to induce lower interest rates and thereby help to account for interest rates moving in ways broadly consistent with an Austrian view of a cyclical expansion.

The path of real interest rates immediately preceding and during the 1990–1991 recession, however, does not provide unambiguously strong support for the ATBC. The ATBC holds that real rates should rise at the cyclical peak. As shown in Table 2 below, the inflation-adjusted rate on AAA bonds trends downward from the peak year of 1988 until the second quarter of 1990, which roughly coincides with the National Bureau of Economic Research's dating of the onset of the contraction. With the exception of the first

Table 2: Long-Term Interest Rates

Year (quarter)	Nominal AAA Bond Rate	Ex Post Real Rate
1988 (1)	9.56	5.96
(2)	9.81	5.41
(3)	9.96	4.86
(4)	9.51	5.61
1989 (1)	9.67	4.27
(2)	9.49	4.89
(3)	8.97	5.17
(4)	8.89	5.19
1990 (1)	9.19	4.79
(2)	9.39	4.59
(3)	9.40	4.70
(4)	9.29	5.39
1991 (1)	8.93	3.63
(2)	8.91	5.41
(3)	8.79	6.39
(4)	8.45	6.05
1992 (1)	8.28	5.18
(2)	8.28	5.58
(3)	7.98	5.98
(4)	8.02	5.72

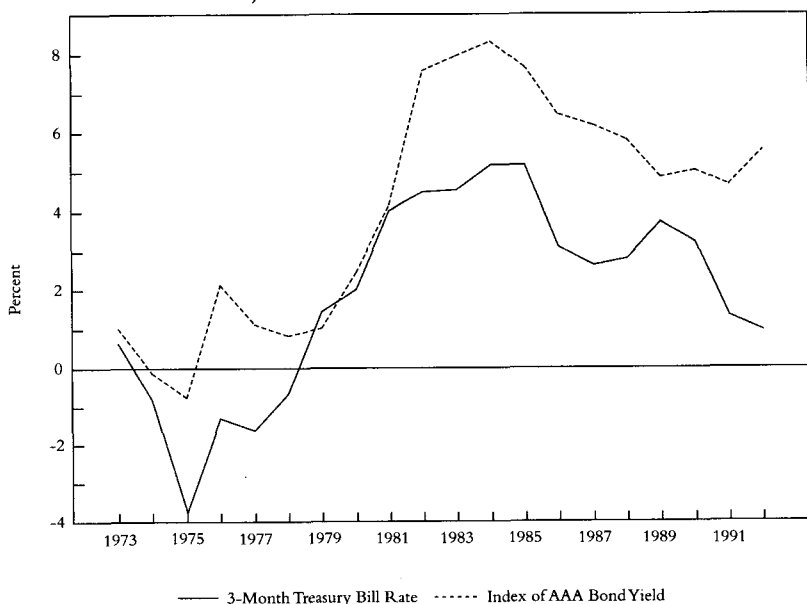
Nominal rates are averages of monthly values of Moody's bond yield index; ex post real rates were calculated by subtracting the quarterly percentage change in the GDP deflator from the corresponding nominal value.

Sources: Federal Reserve Board of Governors and Bureau of Economic Analysis.

quarter of 1991, real rates increased from the third quarter of 1990 to the third quarter of 1991.³⁰ While this general pattern is consistent with the Austrian theory, the fact that real rates continued to rise for two quarters after the recession ended does not square well with the ATBC.

Overall, the level and movement of interest rates during the 1980s is ambiguous in its support of the ATBC. The evidence does not appear decisive enough to warrant confident conclusions.³¹ Indeed, as Figure 4 indicates, the movement of ex post real long-term rates seems to follow a path since 1971 that is not strongly consistent with that suggested by the ATBC. During the 1970s, for example, real long-term rates fell dramatically during the 1973-1975 recession and rose sharply during the first year of the 1975-1979 expansion before falling in 1976. On the other hand, the sharp increase in real long-term rates beginning in 1980 and continuing through 1982, followed by their leveling off and decline thereafter, are consistent with the Austrian cycle theory.

Figure 4: Ex Post Real Interest Rates (nominal rate minus percent change in GDP deflator)



Sources: Federal Reserve Board of Governors; Moody's; Bureau of Economic Analysis.

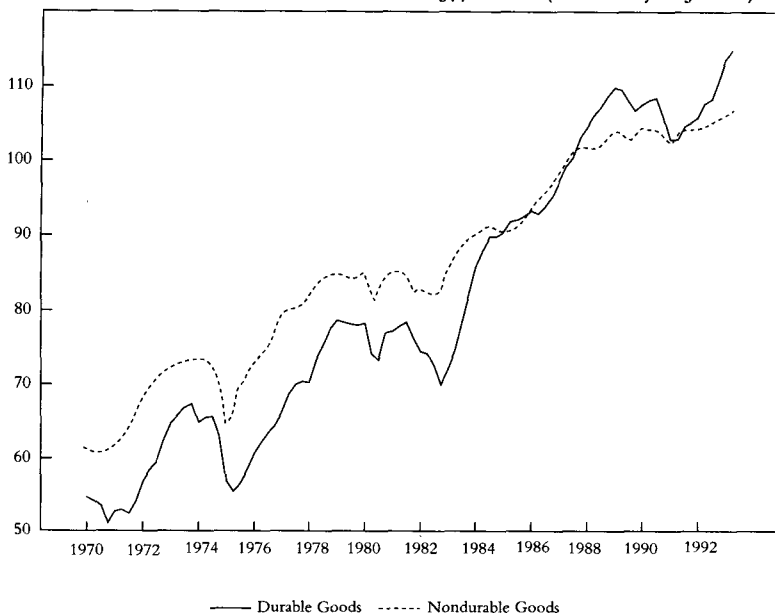
In any case, there are clear pitfalls in making strong inferences based on the movement of calculated real interest rates. This is especially the case when the purchasing power of money is unstable, as has been the case during the past two decades. Inflation, because it creates unsystematic and unpredictable changes in relative prices, interferes with their informational role, thereby making economic calculation difficult. This applies with special force to “time markets,” in which the efficacy of current decisions is decisively linked to a future made even more uncertain by inflation. As Figure 4 shows, for example, during the mid-1970s inflation-adjusted interest rates were actually negative, despite the fact that economic theory tells us that real interest rates should always be positive. Most likely, inflation “surprises” explain the anomalous path of ex post real interest rates during the mid-1970s. Conversely, the rather high ex post real rates of the early 1980s may have been caused by an excessive sensitivity to inflation uncertainties: investors burned by very low real rates of return in the 1970s required a very high nominal return to offset the risk of misforecasting future inflation. In short, despite rational behavior by market transactors, inflation may generate widely distorted market results.³² Not only inflation but other developments during the 1980s also may have affected the path and levels of interest rates during the 1980s. How such factors affect the validity of the ATBC will be discussed below.³³

Prices and Production

The monetary and financial data seem mixed in terms of their consistency with the ATBC. The picture is somewhat clearer when we look at the patterns of production and relative prices during the 1980s. According to the ATBC, the data should display disproportionately large fluctuations in capital and durable goods production relative to consumer goods production during the expansion and contraction of the economy and a procyclical movement in the ratio of capital goods prices to consumer goods prices.

Figures 5 and 6 use data on durable and nondurable goods production as a way to examine the pattern of greater and lesser capital intensive production. First, the data show that the production of durable goods correlated closely with cyclical movements in the

Figure 5: Index of Industrial Production; 1977 = 100 (seasonally adjusted)

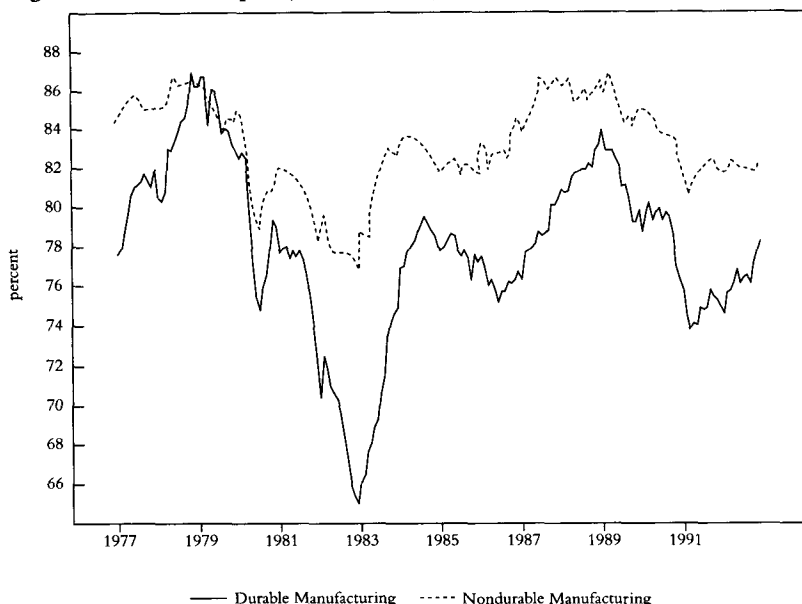


Source: Federal Reserve Board of Governors.

economy. Thus, for example, in Figure 5 the index of durable goods production declines markedly during the 1973-1975 and 1981-1982 recessions. Second, the data also show a larger variance of fluctuation in durable goods production compared to nondurable goods. Both of these patterns corroborate the ATBC's predictions about the magnified effect on capital-intensive production during the cyclical upswing and downswing that an artificial credit expansion induces.³⁴

Figure 7 provides data that suggest the secular trend toward a service economy,³⁵ but it is the fluctuations and turning points in employment in durable versus nondurable goods production that is of special interest to the ATBC. First, as Figure 7 shows, the cyclical variance in the ratio of total employment to employment in durable goods industries is greater than that for total employment to nondurable goods employment. This pattern is consistent with the ATBC because the theory calls for capital-intensive (i.e., durable goods) production and employment to undergo relatively greater fluctuation compared to less capital-intensive activity.

Figure 6: Index of Capacity Utilization (seasonally adjusted)

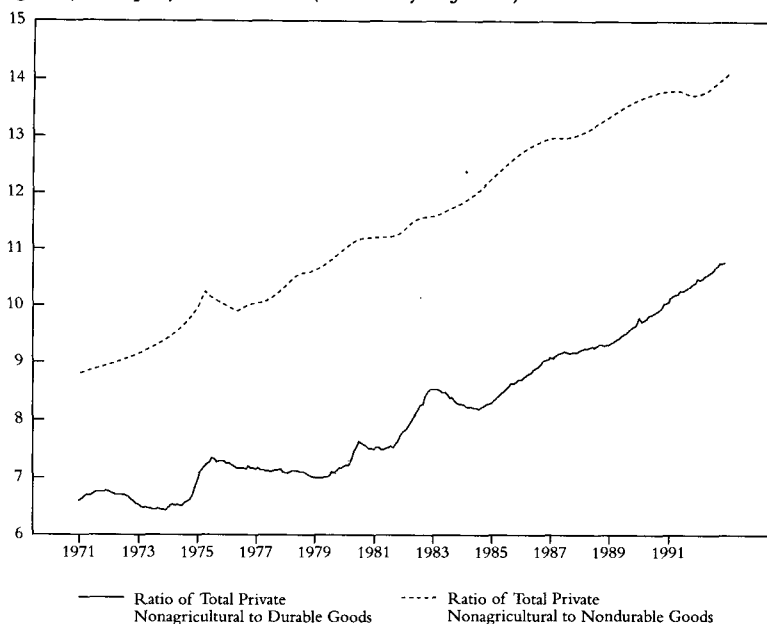


Source: Federal Reserve Board of Governors.

Thus, for example, the 1973–1975 cyclical peak to trough percentage swing for employment in durable goods–manufacturing industries was 11.3 percent but was only 8.2 percent for employment in non-durable goods–manufacturing industries. The pattern for 1973–1975 is typical for the entire period shown, though fluctuations in both ratios become more modest near the end of the decade, a matter discussed below.

Second, as Figure 7 indicates, the ratio of total private nonagricultural employment to employment in durable goods–producing industries increases sharply during all the contractions since 1973—as the ATBC would lead us to expect—with the exception of the 1990–1991 recession. While 1990–1991 may appear to anomolous, a closer look at the data sustains a somewhat different interpretation. Thus, Figure 8 shows that prior to and during the 1990–1991 recession, employment in durable goods industries dropped significantly in absolute terms and relatively compared with employment in nondurable goods industries. For example, between 1989 and 1991 employment in durable goods industries fell by 10.4 percent, while

Figure 7: Employment Ratios (seasonally adjusted)

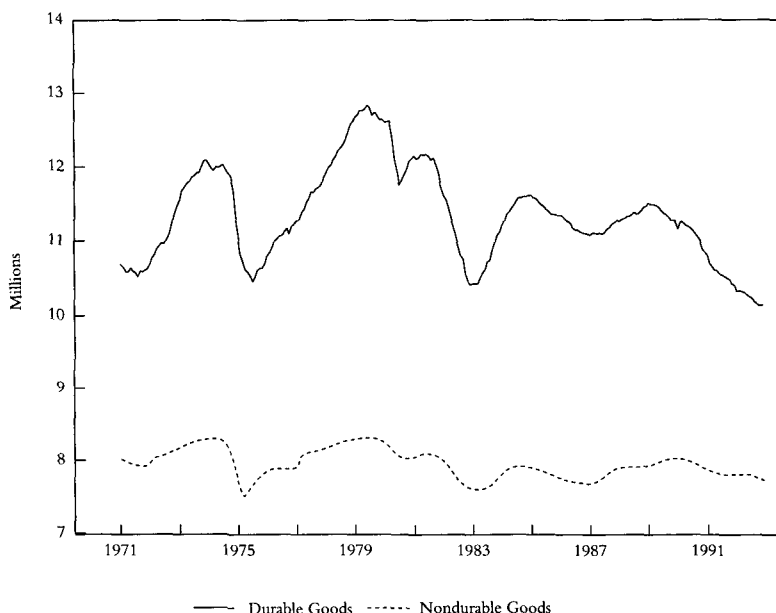


Source: Bureau of Labor Statistics.

only by 2.2 percent in nondurable goods industries.³⁶ Thus, even though (contra the ATBC) the ratio of total to durable goods employment did not rise appreciably during 1990–1991, the actual decline of manufacturing employment in durable goods producing industries supports the ATBC. Still, as Figure 8 indicates, the 1990–1991 recession evidences a smaller amplitude (measured from cyclical peak to trough) and a more gradual decline in durable goods industries employment than the 1973–1975 or 1981–1982 contractions. Overall, therefore, the evidence from 1990–1991 is consistent with the ATBC, but less so than in previous cycles.³⁷

A similar conclusion is also suggested by the data on manufacturers' new orders and shipments in nondefense capital goods industries. As seen in Figures 9 and 10, the pattern of new orders and shipments tracks overall economic activity reasonably well, falling during macroeconomic contractions and increasing during expansions, especially during the 1973–1975 and 1981–1982 recessions. In the 1990–1991 recession, new orders for capital goods decline, while shipments of capital goods generally level off but do not substan-

Figure 8: Employment in Manufacturing Industries (seasonally adjusted)

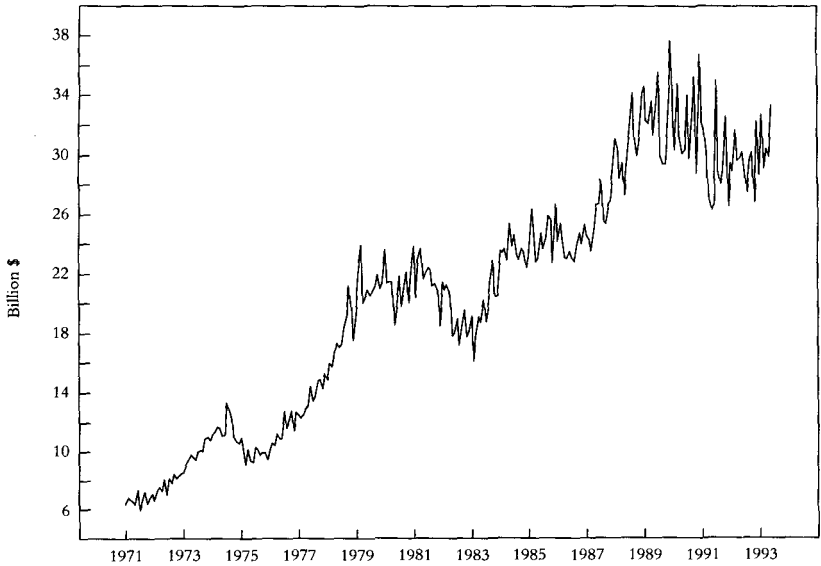


Source: Bureau of Labor Statistics.

tially decline. According to the ATBC, new orders and shipments of capital goods should decline during a recession as resources are reallocated back toward less capital-intensive production methods. The movements in both these series are not inconsistent with the ATBC, though they are somewhat muted.

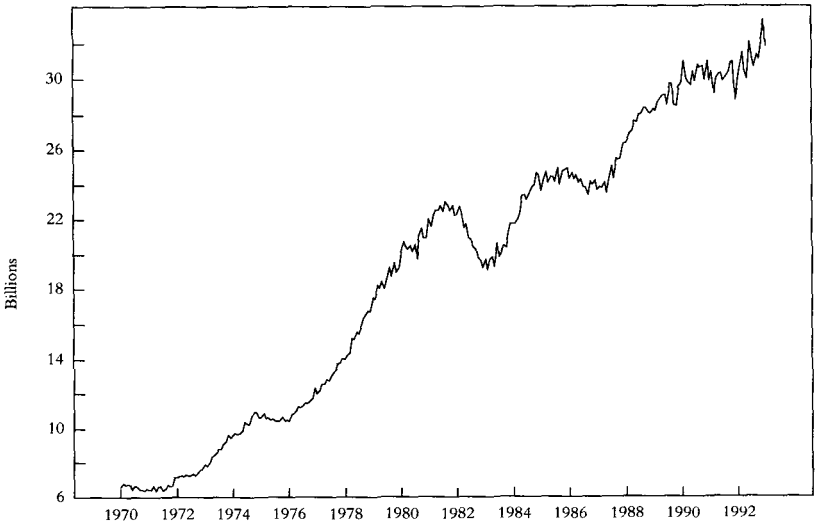
Finally, the Austrian theory holds that the ratio of capital goods prices to consumer goods prices should decline at the onset of the economy's downturn. It is important to recall that the ATBC's emphasis on relative price movements differentiates it from other business-cycle theories. Thus, evidence regarding such movements allows us to determine if the Austrian approach provides insight when other competing theories do not. The ratio's decline signifies the adjustment process of resource reallocation back toward less roundabout methods of production in response to higher rates of profit in consumer goods industries compared to capital goods-producing industries. As Figure 11 shows, the ratio generally follows this predicted pattern, peaking near the onset and then declining during the 1981-1982 and 1990-1991 recessions.³⁸ The ratio does

Figure 9: Manufacturers' New Orders in Nondefense Capital Goods Industries



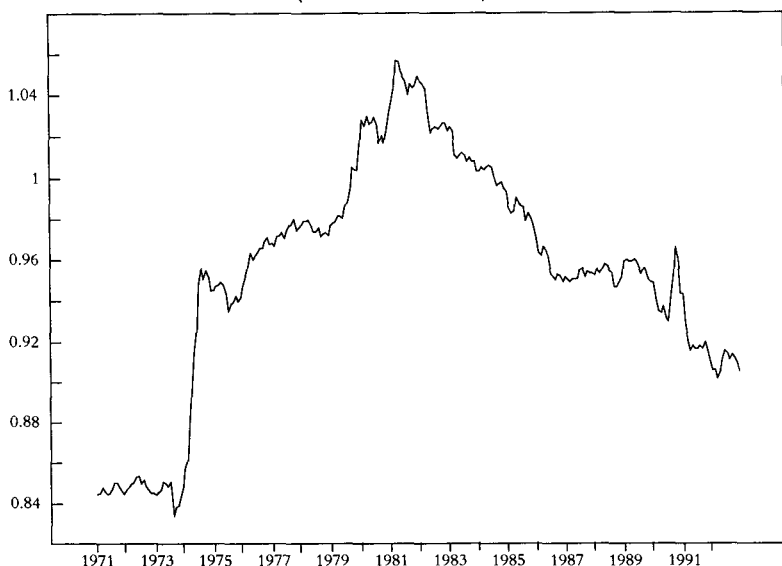
Source: Census Bureau.

Figure 10: Manufacturers' Shipments in Nondefense Capital Goods Industries



Source: Census Bureau.

Figure 11: Ratio of Producer Price Index (Industrial Commodities) to Consumer Price Index (All Commodities)



Source: Bureau of Labor Statistics.

not as closely follow its expected path during roughly the last 18 months of 1973–1975 recession, although its decline in mid-1973 is consistent with the ATBC.³⁹ Overall, however, the ATBC's predictions are supported by the data.

What Happened in the 1980s?

The empirical evidence marshalled to examine the 1990–1991 recession provides positive but not decisive support for the ATBC. The cyclical process that began at the end of 1982 seems to have approximately followed a path consistent with the Austrian theory. Of special significance is the fact that this economic expansion (like that preceding the Great Depression) did not require price inflation. Indeed, the rate of inflation (as measured by the GDP deflator) fell sharply during 1982–1983 and generally remained within a range of 3–4 percent during 1983–1992, although it should be noted that inflation increased by about two percentage points between 1986 and 1989. When a recession finally took hold near the end of the

decade, the economy went through a contraction that suggests a kind of muted Austrian cyclical downturn. The empirical evidence, though not strongly supporting a typical Austrian cyclical downturn, is clearly not inconsistent with the patterns the ATBC suggests ought to show up. In short, an Austrian cyclical process may have been at work during the 1980s, but if so, other factors also seem to have been present.

It should not be surprising that the empirical evidence presented above does not corroborate the ATBC more decisively. Common sense and casual observation tell us that the economy's path is shaped by many influences beyond those suggested by the ATBC. This applies in varying degrees to all cyclical episodes but perhaps with special significance to that of the 1980s. A brief look at some of these factors illustrates their importance in understanding the past cyclical process.

Roger Garrison's paper in this issue is especially helpful because it explains how fiscal deficits and bank instabilities generated a supernormal risk premium that nonetheless was not reflected in market interest rates.⁴⁰ This was accomplished by the Federal Reserve pushing nominal rates below what their true risk-adjusted levels would have required given the uncertainties caused by budgetary deficits and the increased riskiness of bank behavior under the aegis of banking regulatory policies. According to Garrison, actual economic risks were subsidized and externalized by a compliant Federal Reserve (given its ability to monetize Treasury debt) that abetted chronically large government deficits and perverse risk-inducing and risk-disguising federal bank regulations and policies.

The elements emphasized by Garrison are clearly important in understanding the special conditions underlying the expansion of the 1980s and accounting for the artificial lowering of interest rates. There are, however, other circumstances that also played significant roles during this past cyclical episode. An especially important one is the linkage between fiscal policy and economic activity by means of tax policy. As noted earlier, the 1981 Economic Recovery Tax Act (ERTA) significantly stimulated the commercial and residential real-estate market. The resulting real-estate boom led to serious overbuilding and price increases in real estate, especially in some geographic areas.⁴¹ The real-estate boom also contributed to the longstanding but deepening difficulties of the S & L industry as it

attempted to salvage itself by participating in an increasingly speculative and artificially sustained market. Though reeling from the recessions of 1980 and 1981–1982, and despite the fact that by 1983 perhaps half of them had negative net worth, the S & L's continued to invest aggressively in real estate, encouraged by the perverse structure of incentives and support mechanisms for risk taking under federal deposit insurance and the various agencies administering the attendant collection of rules and regulations.

In 1986, however, the Tax Reform Act reversed ERTA's provisions by extending the depreciation period for commercial real estate from 15 to almost 32 years and by eliminating the preferential tax treatment of capital gains. In addition, the 1986 Act reduced the top marginal tax rate from 50 percent to 33 percent, thus reducing the tax savings per dollar of deduction associated with depreciation costs or real-estate losses. The overall effect of the 1986 legislation was to eliminate the tax incentive to hold commercial real estate. This dramatic change in tax policy was the primary initiating factor in the collapse of the real-estate market that began in 1987. Changes in the tax laws plus the downward pressure on prices due to several years of excessive commercial construction and by asset selloffs from "resolved" S & L's generated strong contractionary forces in the commercial and residential real-estate markets.

These effects rippled through the economy, creating large regional pockets of recession. The magnitude of these regionalized contractions tends to presage a somewhat more general slowing down in the overall economy by 1988–1989. This may be seen in the movements of such cyclical indicators as commercial and industrial bank loans (Figure 2), the Index of Industrial Production (Figure 5), the Index of Capacity Utilization in Durable Goods Industries (Figure 6), and employment in durable goods manufacturing industries (Figure 8). Although the NBER dates the recession to July of 1990, a downturn probably began at least one year earlier.⁴² This downturn, though not pervasive enough to qualify as an "official" recession, began with the collapse of the real-estate boom.

There may be, however, some irony in this story. Suppose that, as the ATBC holds, the economy had to go through a corrective process following the expansion of the 1980s. Even so, the precise pattern and manifestation of that process is not strictly determined *a priori*; instead, this process is shaped and contoured by the interplay of unique economic events and fiscal and monetary policies.

This seems to be the case for the 1980s cyclical episode in that one aspect of fiscal policy (the 1986 Tax Reform Act) may have inadvertently induced a mini-recession. The Federal Reserve, moreover, contributed to this contractionary environment, as reflected in the growth rate of bank reserves (see Figure 1) and as suggested both by the sharp decline in the growth rate of M2 in 1987 and by the large increases in the federal funds rate starting in early 1988. Fiscal policymakers simply (and laudably) wanted to lower marginal tax rates and remove tax shelters and loopholes. Meanwhile, in 1987 the Federal Reserve reacted primarily to an unexpected jump in inflation. The unintended consequences of this combination of fiscal and monetary policies allowed the economy to go through its necessary adjustment process but in a prolonged and relatively mild way. Ironically, the often mentioned "soft-landing" objective of discretionary fiscal and monetary policy may have been achieved; but, if so, it was fully inadvertent.

Despite these complexities, the economic data are not inconsistent with the patterns implied by the Austrian theory of the business cycle. Although this suggests that an Austrian-type cyclical process may have occurred during the 1980s, the "fit" was not tight for all categories of data examined here. The data are, however, well explained by a combination of the ATBC and other factors discussed above.

The significance of other influences is neither surprising nor inconsistent with Austrian economics. The cycle theory advanced by Austrian economists emphasizes that distortions in "time markets" lead to misallocations of resources. But it does not preclude or marginalize the possible role of an indefinite array of other circumstances, such as the S & L crisis, the underpricing of risk emphasized by Garrison, or fiscal policy and legislation. In this paper I have highlighted specific aspects of fiscal policy as important agents in accounting for the cyclical process during the 1980s. If there is a lesson here, it seems to have two parts: first, that breakdowns in economic coordination appear in many guises and arise from many causes; second, that the ATBC does indeed add to our understanding of the boom of the 1980s and the recession from which we are still recovering.

NOTES

1. The National Bureau of Economic Research (NBER) cyclical expansion and contraction chronology is used in this paper as a convenient way to present, discuss, and analyze business cycles. Doing so is not without certain hazards. See Murray N. Rothbard, *America's Great Depression* (Princeton: D. Van Nostrand, 1963), 1-7; and Charles Edward Wainhouse, "Hayek's Theory of the Trade Cycle: The Evidence from the Time Series" (unpublished Ph.D. dissertation, New York University, 1984), chs. 5, 6. This notwithstanding, the four NBER post-1973 contractions seem to correspond reasonably well to a broad array of empirical data typically associated with cyclical downturns. However, as discussed below, the NBER approach does not necessarily pick up all relevant cyclical patterns.
2. As reported in *The Wall Street Journal* of September 1, 1993, Department of Commerce figures released in August 1993 reflect substantial revisions over earlier statistics. The revised data indicate that growth in gross domestic product (GDP) during the recession decreased about .5 percent less than previously thought while the growth in GDP during 1992 was about .5 percent more than previously estimated. On the other hand, as reported in the *Wall Street Journal* of September 2, 1993, the Clinton administration recently lowered its GDP growth forecast for 1993 from 3.1 percent to 2 percent.
3. A variety of factors may explain the slowness of the recent recovery. Some of these are associated with legislation that increase labor costs or restrict the functioning of labor markets, such as higher social security taxes, the Americans with Disabilities Act, and the Family Leave Act. The Bush tax increases of 1990 also probably adversely affected the recovery, especially when it became evident that the tax increases were not going to reduce budget deficits and, as a result, that raising future taxes might again be promoted for deficit reduction.
4. The discussion here should in no sense be construed to suggest that monetary policymakers have the capability to "fine tune" the economy.
5. See Knut Wicksell, *Interest and Prices* (New York: Augustus M. Kelley, 1965), and idem, *Lectures on Political Economy* (New York: Augustus M. Kelley, 1966). Elaborations and extensions of Wicksell's monetary theory may be found in Gunnar Myrdal, *Monetary Equilibrium* (London: William Hodge, 1939); and Erik Lundberg, *Studies in the Theory of Economic Expansion* (New York: Kelley and Millman, 1955). On the Austrian theory of capital, the *locus classicus* is Eugen von Böhm-Bawerk, *Capital and Interest* (South Holland, Ill.: Libertarian Press, 1959). Also see Friedrich A. Hayek, *The Pure Theory of Capital* (Chicago: University of Chicago Press, 1941); and Roger Garrison's chapter in Gerald O'Driscoll and Mario Rizzo, *The Economics of Time and Ignorance* (New York: Basil Blackwell, 1985), for more modern treatments.
6. Defining the natural rate of interest when capital is heterogenous involves

serious conceptual difficulties. In a Walrasian model where money has no special or distinct role but is only a numeraire, there would be (n) number of rates of return on capital for (n) number of capital goods. On this, see Friedrich A. Hayek, "Intertemporal Price Equilibrium and Movements in the Value of Money," in *Money, Capital, and Fluctuations: Early Essays*, ed. Roy McCloughry (Chicago: University of Chicago Press, 1984), 79-82, and idem, *The Pure Theory of Capital*, 34-35. John Maynard Keynes, *The General Theory of Interest, Employment, and Money* (London: Macmillan, 1936), 242-43, claimed that a natural rate of interest was associated with each level of employment. On Keynes's criticism of Wicksell, see Gary Mongovi, "Keynes, Hayek and Sraffa: On the Origins of Chapter 17 of *The General Theory*," *Economie Appliquée* 43, no. 2 (1990): 131-56; and Raymond Majewski, "The Hayek Challenge and the Origins of Chapter 17 of Keynes' *General Theory*," in Harold Hagemann and Otto Steiger, eds., *Keynes' General Theory after Fifty Years* (Berlin: Duncker and Humblot, 1988), 99-112. Meir Kohn provides a Keynes-type Wicksellian analysis in "A Loanable Funds Theory of Unemployment and Monetary Disequilibrium," *American Economic Review* 76, no. 5 (December 1981): 859-79.

7. But see Myrdal, n5 above; and Hayek, *Monetary Theory and the Trade Cycle* (Clifton, N.J.: Augustus M. Kelley, 1975), for criticism of the "constant price level" condition.
8. Note that a cumulative expansion (or contraction) implies investment greater (or less) than saving. Wicksell's analysis gave rise to a whole class of cycle models during the interwar years known as "savings-investment" theories. In that sense, the ATBC is one species of a genus that included theories advanced by many leading economists of the time. See, e.g., Dennis Robertson, *A Study of Industrial Fluctuations* (London: P.S. King, 1915), and idem, *Banking and the Price Level* (London: P.S. King, 1926); John Maynard Keynes, *A Treatise on Money* (London: Macmillan, 1930); in addition to Myrdal, n5 above; and Lundberg, n5 above.
9. Ludwig von Mises, *The Theory of Money and Credit* (Irvington-on-Hudson, N.Y.: The Foundation for Economic Education, 1971).
10. The explicitly "micro" orientation of the ATBC contrasts sharply with other standard business cycle approaches. For example, in *The General Theory* Keynes made "the theory of output as a whole" the domain for his theoretical inquiry. The resulting emphasis on economic aggregates to the neglect of their market level underpinnings forms much of the analytical approach of post-World War II macromonetary economics. Among other things this approach tends to give credence to the "flat tire" theory of economy fluctuations popular with the early Keynesians: a recession is analogous to air escaping from a tire, while expansion and stimulative macroeconomic policy pumps the tire back up. It is also useful to note that "quantity theory" analyses employing $MV = PQ$ type framework cannot treat questions of relative price changes (and hence of resource allocation) while retaining consistency in their aggregation structures. This particular

technical problem confronting monocommodity models is an implication of Hicks's "composite commodity theorem." See John Hicks, *Value and Capital* (London: Oxford University Press, 1939), 33–34, 312–13. My thrust here is that quantity theory comparative static exercises must assume relative price invariance to changes in the quantity of money.

11. Some qualifiers are necessary here: (1) as a theoretical matter, any significant and prolonged supply-driven increase in bank credit is sufficient to initiate a Mises-Hayek cyclical process; (2) following Mises, Hayek, and other Austrian theorists, I also assume that as a behavioral generalization modern economies are characterized by central banks which often engage in policies that intentionally or otherwise result in significant credit expansions; (3) the claim that a central bank can affect the domestic supply of credit via control over bank reserves does not imply that the quantity of credit and market rates of interest cannot be strongly affected by other players and institutions.
12. According to Friedrich A. Hayek, "Three Elucidations of the Ricardo Effect," *Journal of Political Economy* 77 (March/April 1969): 282, the ATBC is relevant to a "typical nineteenth century business cycle" characterized by an investment boom.
13. Fritz Machlup, "Hayek's Contributions to Economics," in idem, ed., *Essays on Hayek* (Hillsdale, Mich.: Hillsdale College Press, 1976), 23.
14. An anonymous referee has raised the issue of whether changes in credit flows resulting from an autonomous shift in individuals' intertemporal saving/consumption plans can be distinguished from those caused by central bank interventions. According to the ATBC, the conditions necessary to initiate an Austrian cyclical process require that the inverse comovement of real interest rates and central bank-induced credit expansion are unrelated to the supply of savings. Wainhouse, *ni* above, examines this by testing whether changes in savings were independent of changes in the supply of bank credit during 1959–1981. His findings support the hypothesis. Also see Gerald P. O'Driscoll, *Economics as a Coordination Problem: The Contributions of Friedrich A. Hayek* (Kansas City: Sheed Andrews and McMeel, 1977), 51–55, 70–82, for an excellent treatment of the ATBC.
15. It is no exaggeration to say that contemporary professional judgment sees the ATBC as clearly outside the mainstream. Despite the Austrian revival during the past 20 years, the cycle theory has not shared in a similar groundswell of acceptance or interest. Indeed, it has aroused strong criticism even from otherwise sympathetic "fellow-travelers." Leland Yeager, "The Significance of Monetary Disequilibrium," *Cato Journal* 6, no. 2 (Fall 1986): 369–99, argues that the ATBC should be excised from Austrian economics and replaced with a monetary disequilibrium theory of fluctuations. Yeager claims that the ATBC is "conceivable but incomplete" because it only "lamely" explains the depression phase, is "unnecessarily specific" in its suppositions, and is empirically uncorroborated (380). I do not address Yeager's points here. However, in a paper presented at the

Atlantic Economic Conference in October 1992, “The Consequences of Monetary Disequilibrium in Alternative Macroeconomic Theories,” Steven Horwitz provides an insightful analysis of the compatibility of the ATBC and monetary disequilibrium theory.

16. Foremost among scholarly works analyzing cyclical historical episodes from an Austrian perspective are Murray N. Rothbard, *America's Great Depression*, n1 above; C. A. Phillips, T. F. McManus, and R. W. Nelson, *Banking and the Business Cycle* (New York: Macmillan, 1937); and Lionel Robbins, *The Great Depression* (New York: Macmillan, 1934). Benjamin M. Anderson's *Economics and the Public Welfare* (New York: D. Van Nostrand, 1949), though not explicitly Austrian, is broadly compatible with the ATBC. More recently, Wainhouse, n1 above, has produced an econometric study empirically testing ATBC during the 1959–1981 period. Wainhouse's unduly neglected work points out a significant problem in historical studies based on the ATBC: identifying empirically relevant correspondents to its theoretical concepts. A specific instance of this problem relevant to this paper is the dating of “Austrian” as opposed to “NBER” cycles. See n1 above.
17. The study here does not pretend to empirically test the ATBC. However, since the ATBC purports to offer an understanding of cyclical phenomena, the theory's central propositions should in some sense be reflected in the data.
18. The 5.5 percent rate of unemployment in 1990 was the lowest since 1973 with the exception of 5.3 percent in 1989. All data are from the 1993 *Economic Report of the President* (Washington, D.C.: U.S. Government Printing Office, 1993).
19. The economically relevant interest rate measure is the expected real rate of interest, defined as the nominal (or market) rate minus the expected rate of inflation. Since only the market rate is observable, empirically measuring the expected real rate requires making claims about investors' expectations of inflation. Moreover, since interest income is subject to income taxation (except, of course, for municipal securities), the actual after-tax interest return is the nominal rate times one minus the marginal income tax rate. On the other hand, since interest payments are typically treated as tax-deductible business expenses, the after-tax interest cost is also described by the preceding formula. Note that as the marginal rate of taxation falls, so too does the after-tax cost of and return on capital.
20. See Mises, n9 above, 362–63.
21. The data portrayed in all figures and tables are available on request from the author. The reader will find it helpful to keep in mind the NBER expansion/contraction chronology given in Table 1 when examining the charts.
22. Total reserves equal borrowed and nonborrowed reserves held by depository institutions. Borrowed reserves are created when the Federal Reserve lends reserves (generally “on demand”) to depository institutions. Non-

borrowed reserves are created when the Federal Reserve buys government securities on the open market. Ordinarily, borrowed reserves are a small fraction (about 3 percent) of total reserves. Nonborrowed reserves more closely reflect Federal Reserve policymaking decisions. Although other non-Federal Reserve factors may affect the quantity of bank reserves (including Treasury operations and international financial flows), it is correct to assume that reserves are controllable by the Federal Reserve. How and why the Federal Reserve might choose or not choose to exercise this control raises several important questions pertaining to its formulation and implementation of short- and long-term policy. That analysis, however, falls outside the scope of this paper.

23. The data for nonborrowed reserves mirror very closely that for total reserves.
24. The possible significance of the slowdown in the growth of reserves in 1987–1989 will be discussed below. The average annual increase in total reserves from December 1990 until May 1993 was nearly 10.6 percent. The annualized rate of increase more recently has been even higher (e.g., 15.1 percent from January 1992 through January 1993 and 15.5 percent from January 1993 through May 1993). *Federal Reserve Bulletin* (August 1993, Table 1.20); and *Monetary Trends*, Federal Reserve Bank of St. Louis (June 1993). In addition, it is important to note that the aforementioned rapid increase in reserves has not been reflected recently in similarly large increases in the M2 measure of the stock of money. (See n29 below for a discussion of the various measures of money.) However, it is not necessarily the case that restrictive monetary policy is solely responsible for the slowdown in M2. The Federal Reserve directly controls only about 30 percent of M2. The remaining 70 percent is determined by portfolio decisions of the public and depository institutions. The low growth rate of M2 is mainly accounted for by low growth in its non-M1 components, i.e., those components the Federal Reserve does not directly control, and seems to have been related to a variety of factors, including increased flows away from low-interest-yielding savings-type accounts into bonds and equities, higher bank capital ratios as required under the new Basle Agreement, and higher recently legislated federal deposit insurance premiums. In addition, several researchers have claimed that excessive regulatory oversight beginning in 1989 created a “credit crunch” that seriously dampened bank lending. Weak credit demand associated with the recession is also cited by some as an explanation for the slowdown in M2. See, e.g., Kevin L. Kliesen and John A. Tatom, “The Recent Credit Crunch: The Neglected Dimension,” *Federal Reserve Bank of St. Louis Review* 74, no. 5 (September/October 1992): 18–36. It is important to recognize that those who claim that a credit crunch precipitated the downturn do not usually claim, in contrast to the ATBC, that the bust was the consequence of the Fed’s credit expansion.
25. Curiously, even though commercial and industrial (C & I) loans often

tend to lag behind the general pace of economic activity, they continue to remain below even their lowest 1990-1991 recession values. Thus, during 1992, C & I loans hovered around \$600 billion; during 1993 they have fallen from \$598 billion in January to \$593 in May.

26. Compared with the 1970s, the path of the income velocity of M_1 during the 1980s was highly irregular. Since the dollar value of output (i.e., nominal GDP) must equal the dollar value of expenditures for that output (i.e., the quantity of money times its velocity), erratic movements in velocity tend, other things equal, to make the quantity of money a less reliable predictor of nominal GDP. This, in part, accounts for the Fed's decision in 1987 to cease concentrating on M_1 as a monetary target and to start emphasizing M_2 . Some recent evidence, however, suggests that the expected relationship between M_2 and GDP may also have changed during the past few years. Since 1990, the demand to hold M_2 assets has apparently fallen (i.e., M_2 velocity has increased) despite a decline in the opportunity cost of money as measured by interest rates on M_2 assets. If so, the slow growth in M_2 since late in 1990 (averaging about 2 percent) would not be as much a cause for concern as it would if the relationship between M_2 and GDP were close and more predictable. See Joseph A. Ritter, "The FOMC in 1992: A Monetary Conundrum," *Federal Reserve Bank of St. Louis Review* 75, no. 3 (May/June 1993): 31-49. I thank an anonymous referee for suggesting that I discuss this topic. For a useful overview of the 1980 and 1982 deregulation acts, see Thomas F. Cargill and Gillian G. Garcia, *Financial Reform in the 1980s* (Stanford: Hoover Institution Press, 1985), ch. 5.
27. For a discussion of the tax sheltering opportunities for real estate under different tax regimes, see Lynn E. Browne and Karl E. Case, "How the Commercial Real Estate Boom Undid the Banks," in Lynn E. Browne and Eric S. Rosengren, eds., *Real Estate and the Credit Crunch* (Federal Reserve Bank of Boston Conference Series No. 36, 1992), 57-97.
28. Ex post real interest rates have been calculated by subtracting a measure of inflation from the same time period's nominal rate. While the nominal rate is an observable magnitude, the actual real rate is not. Thus, the calculated real rate is an empirically constructed value whose magnitude depends upon the particular index of inflation. Different price indices generate different values for changes in the value of money. There are also serious theoretical issues (that carry equally serious empirical implications) regarding the formation of inflationary expectations and hence the algorithms appropriate for subtracting inflation from nominal values. For example, the economically relevant expected real rate might be calculated by using a weighted average of past inflation rates to generate forecasts of future inflation. Other approaches could be used with correspondingly different empirical outcomes. Also see n19 above.
29. M_2 is a measure of money comprising M_1 (coins and currency held by the public, travelers checks, demand deposits, deposits in negotiable orders of withdrawal and automatic transfer accounts, and credit union share

drafts), plus overnight Eurodollars and repurchase agreements, plus savings, time, and money market deposit accounts under \$100,000. M2 rather than M1 is now widely used to gauge the quantity of money available for transactions purposes. The Federal Reserve no longer uses M1 either as an indicator or target of policy. The M2 number reported in the text is drawn from data provided by the Board of Governors of the Federal Reserve.

30. The rather surprisingly low real rate for the first quarter of 1991 reflects a drop in the nominal rate of .36 percent and a rather large increase in the inflation rate of 1.4 percent. The strong uptick in inflation seems to have been due in part to the lingering effects of high energy prices related to the Gulf war.
31. Wainhouse's more detailed analysis (n1 above) also finds mixed results regarding the movement of interest rates during the post-World War II period vis-à-vis Austrian cycle theory.
32. The path of nominal rates for AAA bonds actually provides stronger corroborating evidence for the ATBC than the measure of constructed ex post real rates presented in the text.
33. Roger Garrison, "Business Cycles or State Cycles? The Role of Government in Two Boom-Bust Episodes," *CRITICAL REVIEW* 7, nos. 2-3 (Spring-Summer 1993): 259-76, argues that instabilities in the banking sector and uncertainties regarding fiscal policy warranted higher interest rates during the 1980s. This will be discussed below.
34. The prediction of a heightened cyclical variability of production in capital goods industries compared with consumer goods industries is not, as an anonymous referee points out, a claim that distinguishes the ATBC from other business cycle theories. However, it is an implication of the theory of capital underpinning Austrian cycle theory and, as such, we should expect it to show up empirically if the ATBC is to be corroborated.
35. Between 1971 and 1992 total employment increased by 52 percent, by 2.2 percent in goods producing industries, and by 76 percent in service industries. 1993 *Economic Report of the President*, Table B-41.
36. Ibid.
37. Unemployment rates in durable and nondurable goods manufacturing industries during the 1971-1992 period indicate a pattern fully consistent with the discussion of employment in the text.
38. To examine how the ratio of capital goods to consumer goods prices move over the cycle, Figure 11 uses the ratio of the producer price index (PPI) for all industrial commodities to the consumer price index (CPI) for commodities. Strikingly similar patterns also emerge for many other series on producer and consumer prices, such as the ratio of the PPI for all commodities to the CPI for all items and the ratio of the PPI for industrial commodities to the CPI for all items. An anonymous referee also reports that in an examination of 18 more detailed ratios of producer and consumer prices, 10 reveal patterns strongly supporting the ATBC's predictions.
39. The extremely sharp increase in the ratio in 1973-1974 may be explained by

the correspondingly sharp increases in fuel prices, which fed initially into producer prices, and the relatively less severe and delayed increases in commodities prices during that same period. For example, the producer price index for industrial commodities shows a 22.1 percent increase in 1973 from the preceding year, compared to an 11.9 percent increase in the consumer price index for commodities. Moreover, the producer price index for fuel shows year-to-year increases of 10.7 percent in 1973, 33.3 percent in 1974, and 23.3 percent in 1975. Similar increases are found for the entire decade of the 1970s. Only in 1983 does the rate of increase in fuel prices begin to moderate. 1993 *Economic Report of the President*, Tables B-60 and B-61. This pattern in fuel prices may, in part, account for the general upward trend during 1973-82 in the ratio in Figure 11, although the concern in this paper is the pattern of turning points in the ratio.

40. Garrison, n33 above.
41. Overbuilding of commercial properties is reflected in vacancy rates. The Coldwell Banker vacancy index for office space increased from 5.5 in 1982 to 16.3 in 1987, as reported in Donald D. Hester, "Financial Institutions and the Collapse of the Real Estate Boom," in Browne and Rosengren, eds. (n27 above), 114-35 at 127. Also see Olivier Jean Blanchard and Lawrence F. Katz, "Regional Evolutions," *Brookings Papers on Economic Activity* 1 (1992): 1-61, on regional differences in employment growth and unemployment rates between 1947 and 1990 and the charts on p. 63 in the 1993 *Economic Report of the President* for unemployment rates by state for 1982 and 1992. That economic expansions and contractions should involve large regional disparities is quite compatible with the ATBC.
42. George L. Perry and Charles L. Schultze, "Was This Recession Different? Are They All Different?" *Brookings Papers on Economic Activity* 1 (1993): 145-95, provides evidence that the recession may have begun some time during 1989.

THE SAVINGS AND LOAN DEBACLE

The roots of the savings and loan debacle lie in overregulation of the industry resulting from the attempt to promote widespread home ownership. Actions by policymakers unable to admit earlier mistakes compounded the problem throughout the 1980s. Attempts by political decisionmakers to shift blame to the private sector, coupled with a failure to acknowledge the institutional pressures that led congressmen and S & L owners and managers to act as they did, leave taxpayers vulnerable to the repetition of S & L-type crises in other industries.

The crisis that overwhelmed the savings and loan industry during the 1980s seems destined to become one of those defining moments in history—like the Great Depression and the Vietnam War—that influence public policy for a generation or more. In many ways the S & L debacle deserves such attention. In 1979, there were 4,039 thrift institutions.¹ Ten years later 30 percent of the industry had disappeared, leaving only 2,878 institutions, and S & Ls continued to fail.² By September 30, 1993, only 1,964 thrift institutions remained, less than half the number that existed 14 years earlier.³ In addition to wiping out the equity of thousands of S & L owners, the crisis of the 1980s cost taxpayers \$200 billion (before interest).⁴ Surely there are valuable lessons to be learned.

Unfortunately, it is not at all clear that this is happening. Contrary to conventional wisdom, the roots of the S & L crisis are not found in the Reagan presidency or its deregulatory initiatives. The savings and loan industry was already bankrupt in 1980—while Jimmy Carter was president and before any deregulation took place. In Washington and among the media, however, there is still a widespread tendency to blame the S & L failures on deregulation or on fraudulent behavior. The danger is that policymakers who fail to understand or choose to ignore the real causes of the thrift crisis may recreate in other industries the conditions that led to the S & L debacle.

The Roots of the Crisis

S & Ls were shaped for almost 60 years by the government's commitment to promote widespread home ownership.⁵ Unfortunately, many of the policies that made homes more affordable also left S & Ls financially vulnerable.

Savings and loan associations emerged from the Depression as specialized housing lenders given every advantage policymakers could devise. Legislation in 1932 established the Federal Home Loan Bank System. The 12 regional Federal Home Loan Banks were created to provide liquidity to savings and loan associations to avoid disruptions in the housing market. The National Housing Act of 1934 established the Federal Savings and Loan Insurance Corporation to guarantee S & Ls' deposits.⁶ To continue funding mortgages at relatively low costs, S & Ls had to be able to effectively compete with banks for deposits. Tax incentives were used to encourage thrifts to devote their portfolios primarily to mortgages, and interest-rate ceilings on deposits were introduced in 1966 to help control the cost of S & Ls' funds.⁷ Policymakers argued that lower-cost funds would lead to lower mortgage rates.⁸ Finally, until April 1981, federally chartered savings and loan associations were allowed to offer only fixed rate mortgages.⁹ Congress was concerned that shifting some of the interest-rate risk to borrowers through adjustable rate loans would make it more difficult for families to hold onto their houses in periods of rising interest rates.

In hindsight, it is surprising this system lasted as long as it did. By funding 30-year fixed-rate mortgages with passbook savings

deposits, S & Ls mismatched the economic lives of their assets and liabilities, violating one of the most important principles of sound financial management. To make matters worse, the law required that S & Ls' portfolios be concentrated in the regions where they operated.¹⁰ Relatively low inflation rates and limited alternatives for small savers protected the S & Ls until the 1970s, but then things began to change.

The 1970s were marked by increasing economic instability, including dramatic increases in inflation. These were also years of rapid advancement in telecommunications and computer technology. The combination proved the undoing of the savings and loan industry.

Rising Costs

First, the costs of S & Ls began to rise. Faced with double-digit inflation by the end of the decade, S & L (and bank) depositors became increasingly dissatisfied with the low rates of interest available from savings accounts. Depositors had faced similar situations before, when inflation outstripped government-set interest rates, but in earlier periods small savers had few alternatives. Direct investments in the stock or bond markets often required substantial outlays of cash and represented significantly more risk than did insured deposits.¹¹ By the late 1970s, however, the alternative of money-market mutual funds (MMMFs) had come into being.

MMMFs collect the funds of individual savers and invest them in Treasury bills, commercial paper, and even large (over \$100,000) certificates of deposit offered by banks and S & Ls. (Interest rate ceilings did not apply to large CDs in the late 1970s.) As advances in computer technology allowed mutual fund managers to more easily manage many relatively small accounts, minimum investment requirements fell as low as \$1,000. In addition, some MMMFs allowed investors to write a limited number of checks on their accounts. MMMFs were introduced in 1972, but they grew slowly for the first few years. As inflation ballooned and interest rates rose during the late 1970s, however, MMMFs attracted large sums from banks and savings and loans. MMMF assets grew from \$9.5 billion at the end of 1978 to \$236.3 billion in 1982 before declining slightly over the next couple of years.¹² In many cases, S & Ls were forced

to issue large CDs to buy back from MMMFs the funds they had lost through deposit outflows. As Table 1 shows, however, these large (unregulated) CDs cost S & Ls more than 15 percent per year in 1981—a far cry from the $5\frac{1}{4}$ percent paid on passbook savings deposits.

Stagnant Earnings

Meanwhile, S & Ls' earnings stagnated. During the 1950s and 1960s, home owners regularly refinanced their mortgages as they "traded up," moving into new houses every few years. Faced with rapidly rising mortgage rates in the 1970s, this relatively frequent turnover slowed dramatically. The 6 percent fixed-rate mortgage on one's old house looked awfully good when compared with the 15 percent rates on new mortgages. Even when houses were sold, the "assumable" clause in mortgages took on increasing importance. Assumable mortgages allowed the seller of a house to transfer her mortgage contract to the buyer. The buyer thus assumed responsibility for continuing payments under the terms that existed for the previous owner, and S & Ls were limited in their ability to reprice the loans in their portfolios.¹³ High rates on new mortgages also kept many first-time buyers out of the market.

Of course, S & L managers charged substantially higher rates on the new mortgages they did write during the late 1970s and early 1980s. But the depressed housing market coupled with the use of assumable clauses also kept large numbers of the older low-fixed-rate mortgages in S & Ls' portfolios. S & Ls' returns rose only slowly, therefore. As Table 1 demonstrates, average returns for S & Ls' mortgage investments had risen to only 9.58 percent by 1981 while funds raised through large CDs cost almost 16 percent.¹⁴ As the gap widened between their costs and the income from their mortgage portfolios, S & Ls' losses mounted, and the industry watched its capital melt away. By 1980, the industry as a whole was insolvent by more than \$100 billion on a market-value basis.¹⁵ One-third of the industry was reporting losses.¹⁶ Industry lobbyists would soon begin arguing—quite correctly—that S & Ls had gone broke doing exactly what policymakers had asked of them. They had devoted themselves to writing affordable mortgage loans funded with passbook savings deposits, and it had led to disaster.

Table 1: Market Interest Rates vs. Average Yields on Mortgage Portfolios (percent per annum)

Year	Average Interest Paid on Large 3-Month CDs ^a	Average Interest Rates on New Mortgages ^b	Average Returns on Mortgage Portfolios of Insured S & Ls ^b
1975	6.43	9.01	7.10
1976	5.26	8.99	7.20
1977	5.58	9.01	7.35
1978	8.22	9.54	7.72
1979	11.22	10.77	8.21
1980	13.07	12.65	8.80
1981	15.91	14.74	9.58

Sources: a. Edward J. Kane, *The Gathering Crisis in Federal Deposit Insurance* (Cambridge, Mass.: MIT Press, 1985), 78.

b. *Ibid.*, 92.

Few people would argue with the goal of widespread home ownership, but the regulatory methods chosen to promote that goal unintentionally created an increasingly fragile set of financial institutions. Their rigid institutional design left S & Ls incapable of adapting to the increasingly volatile market conditions of the 1970s, and the industry suffered substantial losses as a result. But the story does not end in 1980.

Making the Worst of a Bad Situation

It would take almost 10 years for policymakers to accept that the savings and loan industry could not be returned to its former size and importance. Legislation in 1980 and 1982, as well as regulatory actions throughout the decade, were all directed toward avoiding thrift closures. Administration officials, regulators, and congressmen—Republicans and Democrats alike—were all part of this effort. It is true, of course, that there were individuals who abused the system in both the public and the private sectors, but for the most part the decisions that aggravated the crisis, like those that had led to it, involved no intent to harm or defraud anyone.

By 1980, policymakers knew savings and loan associations had problems. Although there were only 11 thrift failures in 1980, one-third of the industry reported operating losses, and 336 institutions had tangible capital-to-asset ratios under 3 percent.¹⁷ In that year,

Congress passed the Depository Institutions Deregulation and Monetary Control Act, in part because of observed problems among S & Ls. The DIDMCA began phasing out interest-rate ceilings on deposits and authorized federally chartered savings and loan associations to invest in a broader range of assets, including credit-card receivables, corporate-debt securities, and commercial real-estate loans. The DIDMCA also raised the deposit insurance limit from \$40,000 per account to \$100,000 per account for all depository institutions.¹⁸

Nonetheless, conditions among S & Ls continued to deteriorate. The industry as a whole reported operating losses of more than \$8.7 billion in 1981 and 1982,¹⁹ and by the end of 1982, the tangible net worth of 1,300 institutions had fallen to less than 3 percent of their total assets.²⁰ Congress responded by passing the Garn-St Germain Depository Institutions Act. The 1982 legislation further expanded S & Ls' investment powers, and it extended the authority of the Federal Savings and Loan Insurance Corporation to provide direct financial aid to troubled S & Ls.²¹ It was also during this period that Congress signaled the Federal Home Loan Bank Board that capital requirements should be lowered. New accounting standards, known as regulatory accounting principles, were also introduced. These standards allowed S & Ls to delay recognition of losses and include intangible assets in their efforts to report positive capital levels.

From the perspective of 1993, these policies seem extremely short sighted, but in the early 1980s they did not seem all that unreasonable. First, there was the political element. Every congressman had troubled S & Ls in his district, and as leaders of local financial communities, S & L owners and managers had often served on elected officials' campaign finance committees. It might have been difficult for one or two congressmen or for a state or regional delegation to make a case that undercapitalized S & Ls should be allowed to remain open. But when almost all legislators had heard the same story from individuals they knew and trusted, it became much easier to view regulatory leniency as the prudent course of action.

Second, the S & Ls had a good story to tell in the early 1980s. They were clearly the victims of overregulation and fiscal and monetary policies that led to economic shocks beyond their con-

trol. That is the reason early efforts to help the industry took the form of deregulation.

Third, federal decisionmakers had for 50 years viewed the savings and loan industry as a key element in their efforts to promote home ownership, and in the early 1980s, S & Ls were still an important source of mortgage money. To allow a significant portion of the industry to be closed would require an almost complete reversal of the traditional thinking that S & Ls should be promoted and protected. Real-estate brokers and home builders shored up this resistance when they joined the savings and loan industry in arguing that widespread thrift closures would imperil the mortgage market, slowing the housing-construction market and deepening the recession.

Policymakers thus set out to save the S & Ls. The question of what would be best for the country in the long run was, if not unknowable, then at least less important than the more immediate concerns of constituents. Some S & Ls did benefit from the deregulatory initiatives of the early 1980s. In addition, with inflation reduced, interest rates fell and the housing market rebounded. For the industry as a whole, however, losses continued to mount as interest-rate risk was replaced by credit risk. To understand that part of the story, we must turn to the incentives facing S & L owners and managers.

After the 1980 and 1982 legislation began to free S & Ls from past constraints, most S & L executives set out to restore their institutions to financial health. But managers and owners of insolvent firms face incentives that are very different from those faced by the managers and owners of healthy companies. First, the owners of an insolvent firm have nothing left to lose. Second, managers of insolvent firms cannot reduce their liabilities below their assets—that is, they cannot regain their financial health—by pursuing a conservative investment course. Not surprisingly, owners and managers of insolvent companies tend to pursue more risky investments and strategies than do owners and managers of healthy firms. That is why private creditors generally force a firm into bankruptcy when the owners' capital is lost.

Policymakers, however, did not react as private creditors do. By providing financial aid and instituting looser accounting standards, Congress and administration officials allowed many insolvent institutions to remain open. Meanwhile, S & Ls' private creditors—

their depositors — were protected through federal deposit insurance from losses of up to \$100,000 per account. Finally, deregulation allowed owners and managers of insolvent S & Ls to search for higher returns outside the familiar mortgage markets.

Many S & L executives thus began to look for new lending and investment opportunities. Of course, most S & L managers were inexperienced in these new areas, making it difficult for them to accurately assess risk or prudently price new loans. But owners of insolvent institutions had nothing left to lose, and if all went well, their institutions would recoup past losses and regain their financial strength. Indeed, it would have been surprising if managers of insolvent S & Ls had not used their new investment powers to pursue higher returns through increased portfolio risk. When investments soured, S & L executives responded by offering higher interest rates on new deposits. They thus attracted additional funds with which to make new investments promising still higher returns. The industry's interest-rate problem thus became a credit quality problem.

The rapid expansion experienced by many S & Ls bent on outgrowing their problems would not have been possible a decade earlier. While deposit interest rates were strictly controlled, deposit markets were local markets. Depositors had no reason to send funds to S & Ls halfway across the country because everyone paid the same rate. Deregulation of deposit interest rates, coupled with rapidly advancing computer technology, changed that by making possible a nationwide market in deposits. This development was long overdue where small savers were concerned, and it improved the efficiency of U.S. capital markets, but expansive federal deposit insurance put insolvent S & Ls in a position to abuse this new market.

Without deposit insurance, policymakers would not have been able to give S & Ls a second chance. Weak institutions needed continued infusions of funds to pay operating expenses and to support further investments. In the absence of government guarantees, however, depositors would have been unwilling to leave their deposits with bankrupt S & Ls, regardless of the interest rate offered. With federal insurance, depositors were largely unconcerned about the health of the institutions in which they placed their money. As rational investors, they sought to maximize their return for a given level of risk, and all federally insured banks and S & Ls

represented the same level of risk to the depositor. Undercapitalized S & Ls could thus assure themselves a continuing inflow of funds by simply offering to pay slightly higher interest rates than their competitors. Unlike other industries, where failing firms find their funding dries up no matter what price they offer, insolvent S & Ls faced no such constraints. The 1980s were, in fact, marked by "perverse runs" in which funds flowed from stronger banks and S & Ls to the weakest depositories. For S & Ls, the market's natural risk-braking mechanisms were short circuited by federal deposit insurance.

As the industry limped on, many long-time S & L owners and managers simply wanted out. One way out was to sell their S & Ls, and despite the industry's difficulties there were eager buyers.

If someone set out to create an environment that would lure all the crooks and frauds in the country into one industry, he might well use as a model the savings and loan industry of the 1980s. It was easy enough to find disenchanted S & L owners who were willing to sell out for a reasonable price, and once you had an S & L charter, opportunities abounded.

Funding investment schemes was no problem. Federally insured depositors were generally indifferent to the uses to which their savings were put. This made funds effectively unlimited if an S & L was willing to pay a high enough interest rate. The absence of any market discipline meant that the task of overseeing S & Ls' investment decisions fell entirely to government regulators. Even under the best conditions, such overseers rarely show the same healthy skepticism as private investors, but for many S & Ls during the 1980s, the absence of supervision was particularly acute. The regional Home Loan Banks and the FSLIC, responsible for closing bankrupt S & Ls, were overwhelmed. Throughout the mid-1980s, while much of Congress chose to pursue a head in the sand strategy, S & L regulators lacked both the human and the financial resources to deal with the hundreds of insolvent institutions around the country. Examinations were infrequent, and when improprieties were uncovered, examiners found it difficult to follow up in a timely fashion.²²

Despite these conditions, careful analyses have placed the losses caused by outright fraud (as opposed to those caused by the exercise of poor judgment) at only 3 to 10 percent of the total losses.²³ In short, the lion's share of the \$200 billion lost in the S & L disaster

did not come from greed run amok. The money is not stockpiled in secret offshore bank accounts, and aggressive criminal investigations will do less to protect taxpayers than would the abolition of federal deposit insurance and less willingness to prop up undercapitalized depository institutions.

Despite it all, one-third to one-half of the S & L industry remained reasonably healthy throughout the 1980s. Whether these institutions were better capitalized to begin with, more skillfully managed, or just plain lucky has not been fully determined. What is clearer is that the government's response to the insolvent S & Ls undermined the stability of even the well-capitalized institutions.

First, healthy S & Ls were asked to pay increasing deposit insurance premiums to protect depositors in failed institutions. These rising premiums were not risk related.²⁴ Thus, adequately capitalized, conservatively managed institutions effectively subsidized their high-flying brethren, and as premiums rose the cross-subsidy increased.

Second, healthy S & Ls' (and healthy banks') funding costs were increased by the continuing operations of undercapitalized and insolvent institutions. Sick institutions were willing to bid whatever was necessary to keep new deposits coming in the door, and like the "zombies" for which they were named, decapitalized S & Ls fed off of their healthier counterparts.

In properly functioning capital markets, businesses or financial institutions representing less risk are able to attract funds with lower interest rates than their more risky competitors. For federally insured depositors, however, the relevant risk is not the risk that a given bank or S & L will fail. Instead, federal deposit insurance effectively substitutes the government's credit rating for the credit rating of banks and S & Ls. Healthy institutions consequently gain little or no cost advantage from the fact that they are well capitalized. To retain their customers, more conservative S & L executives often must match the higher interest rates paid by weaker institutions. This problem was particularly acute during the 1980s because so many insolvent banks and S & Ls were allowed to remain open.

Healthy S & Ls' problems did not end with higher premiums and interest payments, however. When federal regulators did get around to dealing with insolvent institutions, they often removed bad assets and injected capital before selling them to new investors. S & L executives who had maneuvered through the crisis then

found themselves competing against counterparts revitalized by government monies. Meanwhile, no one offered to relieve solvent S & Ls of the burden imposed by their nonperforming loans.

By 1985, regulators were taking steps to better police some of the worst abuses, and in 1987, Congress officially recognized that the FSLIC had run out of funds. The \$10.7 billion provided to it by the Competitive Equality Banking Act was woefully inadequate, however. When Congress and the Bush administration finally did address the industry's problems in 1989, owners and managers of healthy S & Ls were tarred with the same brush as the frauds.²⁵ The Financial Institutions Reform, Recovery and Enforcement Act of 1989 made few distinctions between healthy and insolvent institutions. Many of the new investment powers granted earlier in the decade were lost to the entire industry, regardless of how prudently or profitably they might have been exercised by some S & L managers.²⁶

In short, government policies aimed at protecting weak and insolvent S & Ls during the 1980s raised the costs of healthier, better-managed institutions. The end result was probably more total failures than would have occurred in the absence of such policies. Furthermore, by treating healthy and unhealthy institutions alike in the cleanup effort, policymakers have forced S & Ls to again concentrate their portfolios in mortgage-related assets.

In a recent book, *The Risk of Economic Crisis*, Martin Feldstein concludes that "the major source of increased risk in our economy has been a series of seemingly well-intentioned government policies."²⁷ The S & L crisis is certainly a case in point. Losses among S & Ls ultimately grew from the government's desire to promote home ownership and from its efforts, begun in the 1930s, to reduce financial risk to savers. The difficulties facing S & Ls were much more complex than the story of fraud and mismanagement commonly told. Unfortunately, most in the media and the public continue to view the S & L crisis as concrete evidence of the failures of deregulation and private greed allowed to run amok. As is often the case, the conventional wisdom is mistaken.

NOTES

1. Philip Bartholomew, *Reforming Federal Deposit Insurance* (Washington, D.C.: Congressional Budget Office, 1990), 145.

2. Lawrence J. White, *The S & L Debacle: Public Policy Lessons for Bank and Thrift Regulation* (New York: Oxford University Press, 1991), 15.
3. Research Department, Federal Deposit Insurance Corporation, October 26, 1993, telephone conversation.
4. Some recent estimates have lowered this number because of falling interest rates and rising prices for some of the assets still held by the Resolution Trust Corporation. Although the ultimate cost of the cleanup may be lower than originally expected, there are still cleanup costs that are unaccounted for.
5. For an excellent, very readable description of how policies designed to help home owners undermined the financial health of S & Ls, see James Bennet, "How the Cleaver Family Destroyed Our S & Ls," *The Washington Monthly*, September 1990: 38-46.
6. For a concise history of S & Ls before 1980, see James R. Barth, *The Great Savings and Loan Debacle* (Washington, D.C.: American Enterprise Institute, 1991), 14-16.
7. *Ibid.*, 19. Regulation Q, which established limits on the interest paid by both banks and S & Ls, allowed S & Ls to pay a slightly higher rate than banks. This "interest rate differential" was designed to divert savings from banks to S & Ls to promote mortgage lending.
8. Small savers were hurt by these interest-rate restrictions, and it is unclear that mortgage rates were, in fact, lowered. Below-market deposit rates reduced the supply of funds available for mortgages below what would have been available otherwise. A reduced supply of savings will tend to raise loan rates, other things equal.
9. *Ibid.*, 127.
10. *Ibid.*, 21. Before 1964, S & Ls were prohibited from making loans secured by property outside a 50-mile radius of the institution's home office. In 1964, the acceptable radius was extended to 100 miles.
11. Before the mid-1960s, some small savers might have fled to short-term Treasury securities. To discourage depositors from leaving banks and S & Ls, the federal government raised the minimum size of Treasury securities from \$1,000 to \$10,000.
12. White, 69. In 1992, MMMFs held over \$500 billion in assets.
13. As with fixed-rate contracts, assumable mortgages were preferred by state and federal policymakers as a way of protecting the interests of home buyers.
14. S & Ls' overall cost of funds would not have been quite this high, as large CDs represent a particularly costly source of money. Still, these comparisons provide some indication of the problems S & Ls faced.
15. Edward J. Kane, *The Gathering Crisis in Federal Deposit Insurance* (Cambridge, Mass.: MIT Press, 1985), 102.
16. White, 70-71.
17. Barth, 27. Tangible accounting principles (TAP) measure net worth according to generally accepted accounting principles (or GAAP), except

that goodwill assets are excluded from the calculation. TAP accounting still does not account for losses not realized because assets are carried at acquisition, rather than market, value. Thus, it still overstates capital in some cases.

18. For a more detailed discussion, see James R. Barth and Martin A. Regalia, "The Evolving Role of Regulation in the Savings and Loan Industry," in Catherine England and Thomas Huertas, eds., *The Financial Services Revolution: Policy Directions for the Future* (Boston: Kluwer Academic Publishers, 1988), 158.
19. White, 70.
20. Barth, 27.
21. For a more detailed description of the 1982 legislation, see Barth and Regalia, 158-59.
22. The number of field examiners for S & Ls was actually reduced slightly between 1981 and 1984. See White, 88.
23. Barth, 44.
24. The 1991 Federal Deposit Insurance Corporation Improvement Act did introduce risk-related deposit insurance premiums, but proposed premiums are only slightly higher for riskier institutions.
25. The S & L crisis is still not "resolved" in the sense that the Resolution Trust Corporation, created by the 1989 Financial Institutions Reform, Recovery and Enforcement Act to close insolvent savings and loans, is still in business. The RTC ran out of money in April 1992, and it was forced to wait until late 1993 before Congress agreed to provide further funds.
26. For a more complete description of FIRREA, see Barth, 79-99, or White, 175-203.
27. Martin Feldstein, "The Risk of Economic Crisis: Introduction," in idem, ed., *The Risk of Economic Crisis* (Chicago: University of Chicago Press, 1991), 17. Feldstein counts the S & L fiasco as one of four economic crises of the 1980s.

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The U.S. banking system, which was built on the Government's promises of "safety" for all depositors, is beginning to look more and more like a giant scam. The *half-trillion dollar* S&L debacle already ranks as the largest financial scandal in human history and may end up costing each of us taxpayers thousands of dollars.

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Hundreds of savings-and-loan institutions failed in the early to mid-1980s. In 1986 the Federal Savings and Loan Insurance Corporation, the agency that promised to repay S & L depositors when their failed institutions could not, itself became insolvent. By the time the federal government faced up to the enormous expense of covering its S & L deposit guarantees (which it did only after the presidential election of 1988 had been concluded), the difference between the FSLIC's obligations and its reserve funds (built up from taxes on deposits) amounted to about \$200 billion, or \$3,200 per average family of four, excluding interest on the additional federal debt incurred. (The final tab is not even in yet, because the assets of insolvent S & Ls have not yet been fully liquidated. The less those assets fetch when sold, the higher the final tab will be.)

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Lawrence H. White, Department of Economics, University of Georgia, Athens, GA 30602, telephone (706) 542-1311, telefax (706) 542-3376, is most recently the editor of *The Crisis in American Banking* (New York University Press, 1993).

Congress was so displeased by this turn of events that in 1989 it abolished the FSLIC and its associated regulatory agency, the Federal Home Loan Bank Board, and created the Resolution Trust Corporation to liquidate the assets of the failed thrift institutions. S & L deposit guarantees (called "deposit insurance," although in many respects they are unlike the contracts sold by insurance companies) became the responsibility of the agency already responsible for guaranteeing commercial bank deposits, the then-solvent Federal Deposit Insurance Corporation.

Meanwhile, hundreds of commercial banks were failing each year from 1984 to 1991, and the average size of the failed banks was growing. The FDIC itself ran out of funds in 1991. Congress desperately put the FDIC on a life-support system by granting it an additional \$70 billion in borrowing authority in November 1991. The FDIC Improvement Act, passed at the same time, required that the FDIC repay any borrowings from the future taxes ("insurance premiums") it would collect or from sales of the assets it would acquire from failed institutions. Whether the FDIC will be able to repay its loans by those means remains to be seen. Yet another bailout of a federal deposit guarantee agency may be in the cards.

The S & L Crisis and the Banking Crisis

How did the U.S. banking industry get into the condition of such systemic weakness? As Salsman (7) rightly observes, "particular banks may get in trouble for reasons that are readily apparent to all (often in hindsight) upon close inspection. But we need to understand why general and widespread weakness in the banking system has developed in recent decades." A number of recent books and articles have examined the details of the savings and loan debacle, and there may soon be a raft of literature on the commercial banking crisis. While popular accounts of the S & L crisis have emphasized cases of mismanagement, fraud, and scandal,¹ economists have persuasively traced the weakness of the S & Ls primarily to regulatory restrictions and to the malincentives created by deposit guarantees.²

The economists' accounts show that the collapse of the S & L industry had long been waiting to happen.³ Legally restricted to

making long-term fixed-rate mortgage loans, and enabled by deposit guarantees to raise funds with short-term deposits, thrift institutions were constrained to bet heavily against a large rise in interest rates.⁴ They lost the bet when interest rates soared between 1978 and 1982. The average interest rate paid on savings deposits in FSLIC-insured institutions rose above 11 percent in 1982. The thrifts' assets consisted largely of conventional fixed-rate mortgage loans made in the 1960s and 1970s, paying between 6 percent and 9 percent. Paying 11 percent to depositors while receiving only 6 to 9 percent from old mortgage borrowers, hundreds of thrifts bled to death.

The thrift crisis deepened, despite the fall in interest rates after 1982, because the authorities failed to close literally hundreds of insolvent thrifts. They chose instead to weaken the regulatory accounting rules, to practice "forbearance," and to gamble that economically insolvent thrifts might climb back into the black. Forbearance created an army of "zombie" thrifts, economically dead but not yet buried, many of which began pursuing high-risk lending strategies. The zombies pursued the financial equivalent of shooting craps in a desperate (but situationally rational) attempt to regain solvency. High-risk lending offered the owners of insolvent thrifts their best hope of making their shares worth something again, with no downside risk, because multiplied losses would fall entirely on the FSLIC and on taxpayers.

The owners and managers of zombie thrifts faced a situation much like that of a casino gambler who finds that his uncle has foolishly guaranteed to cover any debts he incurs in a certain evening. Late in the evening, having piled up debts exceeding his own wealth, the gambler's only hope of leaving the casino with any money is to make long-odds gambles that will bring big payoffs if he's lucky. If he isn't lucky, well, too bad for his uncle, who gets stuck with the tab. The high-roller mentality that swept the thrift industry, and later the banking industry, has been widely disparaged. But bankers were only responding rationally to the perverse incentives before them.

While the thrifts' high-risk lending did pay off in some cases (some gamblers do win at roulette on a given evening), many more went even deeper into the red. Meanwhile, by bidding deposit rates up and loan rates down, the zombies made survival more difficult

for previously solvent thrifts. Some passed into insolvency and became zombies themselves.

The recent failures of record numbers of U.S. commercial banks, and the insolvency of the FDIC, have been widely reported in the business press. The primary immediate cause of bank failures has been defaults on many of the loans banks made in the 1980s. Southwestern banks suffered big real-estate and energy-loan losses in the late 1980s, with Texas and Oklahoma respectively suffering 453 and 113 bank failures in 1982-1990. The major "money-center" banks had to write off large amounts of loans to less-developed countries in 1987 and 1989. New England banks took large losses on real-estate loans in 1989-1991, dramatically illustrated by the closure of the Bank of New England amid depositor runs in January 1991. Commercial banks throughout the country accumulated large inventories of foreclosed real estate during the 1991-1992 recession.⁵

These loan losses, however, are results, not causes, of an unsound banking system. In many respects the wave of failure in American banking resembles a slow-motion replay of the savings and loan debacle described above. Commercial banks, not legally constrained to lend on long-term fixed-rate mortgages, consequently suffered from less of a mismatch between their assets and liabilities than did thrifts, and so were less damaged by the surge in interest rates of 1978-1982. But banks that had issued short-term and demand deposits to finance long-term loans did suffer large losses during the period because short-term rates were above long-term rates. The usual relationship, short rates below long rates, returned in 1983. The number of problem banks nonetheless grew steadily until 1986, principally because business failures and loan losses remained high even beyond the official end of the severe 1980-1982 recession. The increasing number of banks approaching the brink of insolvency meant, as it had with thrifts a few years earlier, that financial "gambling" became increasingly attractive. Because they could pass excess losses to the FDIC, nearly insolvent banks felt free to make high-risk loans. Deposit guarantees and forbearance, in other words, have created in commercial banking the same mal-incentive problems (or "moral hazard") that we saw in the deepening of the thrift-industry crisis. A deepening of the banking crisis was avoided in 1993, in large part because short-term interest rates were unusually far below long-term rates, giving banks an espe-

cially favorable environment, but that environment cannot be expected to last.

Federal Reserve policy was directly responsible for the surge of interest rates in 1978–1982, and indirectly responsible for many of the subsequent loan losses, both of which have caused so much trouble for thrifts and banks. The surge in nominal interest rates primarily reflected a surge in inflationary expectations, which was the financial market's belated response to a decade of increasingly rapid money growth and correspondingly rising inflation. For almost a decade, inflation had outrun interest rates, so that lenders found that the amount they were repaid bought less than the amount they had lent. They naturally began to demand higher inflation premiums in nominal loan rates. In addition, in an effort (eventually successful) to reduce the inflation rate, the Fed in late 1979 began a tight-money policy that drove up interest rates in the short run.

Salsman (20–21) appeals in passing to the role of erratic monetary policy, as one source of loan losses. Unexpected Fed injections of new money into the loanable funds market during the 1970s temporarily reduced real (inflation-adjusted) interest rates. The fall in interest rates misleadingly signaled business borrowers that the real profitability of interest-sensitive investment projects had increased, and so spurred an investment boom. When interest rates later returned to their “natural” levels, investment projects that looked profitable at low rates were abandoned, and bank loans went into default.⁶

The Increasingly Perverse Incentives Created by Federal Deposit Guarantees

Deposit guarantees create a “moral hazard” problem because fully covered depositors do not discipline a weak institution by moving funds to a stronger institution or by demanding higher interest rates on their deposits to compensate for the greater risk of failure by their bank. With its customers fully covered against product failure, and facing no higher FDIC premiums than a sound bank, an unsound bank can sell what amounts to a lower-quality product at the same price. Flat-rate deposit insurance thereby subsidizes risk taking.

Forbearance exacerbates the moral hazard from deposit guarantees. An insolvent bank, left open by forbearance and enabled by deposit guarantees to bid for funds with little or no risk premium, has no better prospect than to "shoot the moon" (e.g., on an unbalanced portfolio of risky energy or real-estate or third-world loans) to try to grow back into the black. It has everything to gain and, with FDIC absorbing the downside risk, nothing to lose. With enough forbearance it can even operate a Ponzi scheme, using the funds from new deposits to pay the interest on old deposits.

The moral hazard problem in U.S. banking intensified during the 1980s because of a shift in bank regulatory policy toward increased forbearance, and because of the expansion and increasing subsidy value of deposit guarantees. Increased forbearance is evident in the fact that during 1980-1985, with fewer annual failures, the typical bank failure was resolved about 15 months after it first appeared on the FDIC's problem list. By 1987-1989, amidst 200 failures per year, the typical failed bank was not resolved for 21-28 months after first being listed.⁷

The most important expansion of coverage came from the FDIC and Federal Reserve's adoption of the so-called too-big-to-fail doctrine, under which even deposit balances over the insured limit (currently \$100,000 per depositor per bank) are de facto protected from any loss when lodged in banks considered particularly important. Under this doctrine, the FDIC increasingly resolved bank failures by arranging takeovers that fully protected all deposits from loss. Until 1984 there had remained the threat that depositors with balances above the insured limit might take losses if a bank was in such bad shape that no purchaser could be found. The FDIC removed even that threat in the case of Continental Illinois, when the FDIC itself assumed ownership (i.e., nationalized) the insolvent bank.

Salsman (62) notes that the Depository Institutions Deregulation and Monetary Control Act of 1980 raised explicit deposit coverage from \$40,000 to \$100,000 per account. A deposit brokerage market subsequently arose, allowing a depositor easily to break an over-limit sum into under-limit deposits to be placed in whatever banks are currently offering the highest rates. This development probably had little effect on thrift and bank risk taking, however, and brokerage would likely have arisen even if the insured limit had not increased. Brokerage lowers a bent-on-growth bank's cost of

rounding up deposits by a small amount, which itself improves the bank's profitability, and was not likely responsible for many banks switching to an aggressive growth strategy. Other methods for drawing in deposits are also available, and an individual U.S. depositor has always had access to unlimited coverage by spreading funds among banks.⁸

The subsidy value of deposit guarantees increased because of the change in the macroeconomic climate in the 1980s. As interest rates became more volatile, banks faced an increased probability of becoming insolvent due to interest-rate movements, but deposit insurance premiums remained the same.⁹

Salsman (56 and 65, tables 7 and 10) provides strong evidence that deposit guarantees have led to increased bank gambling, in the form of data showing the declining capital ratio of the U.S. banking system since 1934. Banks have substituted "public capital" for private capital (30), choosing lower capital/asset ratios as deposit guarantees have grown in scope and in implicit value. A typical bank in the 1920s would advertise the value of its capital by painting it on its front window in gold leaf; today it need only display its FDIC sticker.

Salsman also offers tables showing a declining average reserve ratio as evidence of a weakening banking system, but it is not clear how much that ratio's decline merely reflects the lowering of legal reserve requirements. He might also have examined the recent shift in bank portfolios toward riskier assets. U.S. banks reduced their safe assets—cash and investment securities—to only 27 percent of assets in 1990 from 36 percent in 1980. Relatively risky assets—loans—rose to 61 percent from 54 percent, with particularly risky real-estate loans rising to 23 percent from only 15 percent. Within real estate, banks shifted toward riskier borrowers, namely construction and development loans and commercial mortgages.¹⁰

Just as competition from zombie thrifts impaired the profitability of solvent thrifts, competition from zombie banks has weakened solvent banks. Forbearance has thereby raised long-run resolution costs. The FDIC's policy of resolving institutions with "assistance," absorbing bad assets to keep broken banks open, puts unassisted institutions (which have to swallow their own bad assets) at a competitive disadvantage. The shrinkage of the thrift industry, conversely, has helped strengthen surviving thrifts and commercial banks by giving them cheaper deposits.¹¹

Salsman (71) notes important evidence that even the largest U.S. banks are troubled. In 1980, Moody's Investors Services rated the debt of 14 major banks AAA; today it gives only one U.S. bank that rating. Stock traders' expectations of likely future difficulties in banking are reflected in low share prices (relative to current reported earnings) for all banks, but especially for large banks. While healthier banks trade at slightly above book value, money-center banks have been trading well below (72).

American Banking in Long-Run Perspective

Economic research, as cited in the foregoing synthetic account of the S & L and banking crises, ordinarily analyzes the impact of particular banking regulations one at a time. Salsman draws extensively on these studies, but makes an original contribution by embedding them in a general comparative-institutions framework. Much as an international trade theorist might use the theoretical abstraction of a pure free-trade regime as a benchmark for analyzing the U.S. tariff system as a whole, Salsman uses recent research on the theoretical abstraction of a completely laissez-faire banking system as a benchmark for analyzing the U.S. system of bank regulation as a whole. Where an empirical trade economist might offer evidence by contrasting high-tariff with low-tariff countries, Salsman exploits the relevant contrasts between the current banking regime and historical regimes of less-regulated banking. He finds that the U.S. "free banking" era of 1838-1863 was much sounder, as evidenced by capital-asset and liquidity ratios.

The reader of Salsman's monograph quickly confronts a terminological difficulty. Near the top of his first page the author announces that he will label as "central banking" any departure from laissez faire: "By central banking we mean all forms of government intervention in the banking system, whether a legal tender monopoly on the issue of bank notes, mandatory deposit insurance, or geographic and functional regulation." In some subsequent passages he does indeed associate any and all regulations with "central banking." But elsewhere he adopts the more standard meaning of the term, namely, a regime in which bank-regulatory or monetary-control powers are vested (typically by the central government) in a single financial institution. Farther down page 1 he accordingly

states that “we date the beginning of central banking in the United States to the formation of the Federal Reserve System in 1913.”

As Salsman recognizes, “all forms of government intervention” in the American banking system did not begin with the Federal Reserve Act. Deposit insurance and geographic entry barriers are not currently administered by the Federal Reserve, nor have they been exclusive to the federal government. Contrary to Salsman (17), not all central banking systems “worldwide and throughout history” have exhibited deposit guarantees. So it is hard to see why it is helpful to call deposit insurance and all other interventions elements of “central banking.” Such a usage makes it difficult to know which elements Salsman has in mind in passages where he says sweepingly that “central banking” produces this or that result.

Salsman (25–28) offers the interesting hypothesis that U.S. banks in recent decades may have been directly (as well as indirectly, via the business cycle) weakened by surprisingly expansionary monetary policy. The Fed rapidly expanded the monetary base, leading in textbook fashion to the multiple expansion of bank deposits and loans. Meanwhile, Salsman argues, banks were not able to raise or internally generate enough capital to keep pace, so their capital/asset ratios fell. At least in the 1980s, however, industry aggregates do not show this pattern. Capital growth has been at least as volatile as asset growth.¹²

As many commentators have, Salsman (73–74) notes the secular erosion of the profitability of commercial banking in recent decades. The typical commentator sees “securitization,” and other forms of new competition from nonbank intermediaries and securities markets, as exogenous causes of the reduction in the spread between loan and deposit interest rates and therefore in bank profitability. Salsman, by contrast, cogently observes that in a less-regulated environment such competition would not have the same impact. If banks were allowed to participate fully in securities markets, they too could profit from securitization. If legal barriers had not been erected between banks and thrifts, or banks and non-banks, financial service providers would have been spared the transitional difficulties associated with the erosion of those barriers. The profits lost from the lifting of deposit interest rate ceilings after 1980 were artificial rents created by the ceilings in the first place.

Salsman (113–18) sharply rebuts two fallacious views of the problem popular in Congress and among some onlookers. The first is

that the meltdown of the thrift and banking industries has been due to greedy, bad, and personally dishonest managers, rather than to ill-conceived and badly implemented federal regulations that created perverse incentives for managers. Charles Keating and Neil Bush have come to symbolize the search for scapegoats. Proponents of the "bad apple theory," as Edward Kane calls it,¹³ do not explain what might have caused an epidemic of dishonesty, or how it could possibly have been large enough to account for hundreds of failures and a \$335 billion discrepancy between assets and liabilities. Ironically, the aggressive go-go lending policy of zombie thrifts in the 1980s represented the epitome of responsible management: given the situation created by the regulatory regime, it held out the best hope of increasing the firms' market value.

The second popular, but equally mistaken, diagnosis is that banks and thrifts began to misbehave because Congress loosened regulatory restrictions in the early 1980s. In fact, widespread insolvency among the thrifts came before deregulation, and was a major impetus to it. Bank managers could make as many long-odds loans as they liked even before deregulation. The loosening of activity restrictions actually made it possible for banks and thrifts to become safer, through floating-rate loans and greater diversification, if they had wanted to. The reason they took on more risk in the 1980s was not that restraints loosened, but that their incentives to gamble increased when monetary policy shocks drove many into or close to insolvency while the authorities practiced greater forbearance and expanded the scope of deposit guarantees.

The bankruptcy of the FSLIC and FDIC were thus not accidents due to bad luck or a few bad apples. A regulatory regime that shifted banks' risk-taking losses onto taxpayers encouraged risky banking, however unintentionally. The eventual spread of unsound banking practices was to be expected. As it turned out, the crisis of the deposit guarantee agencies was precipitated by erratic monetary policy, but Salsman (54) notes that the 1980s were not the first instance of such an outcome. Besides the federal government, about 30 state governments have operated deposit guarantee schemes, the earliest dating from 1829. All but a few have gone broke, victims of the moral hazard they have created.

That deposit guarantees have subsidized risky institutions has not been entirely unintentional. The result is consistent with the original motive behind federal deposit guarantee legislation during

the Great Depression. The ostensible motive was to stop bank runs based on fears of bank insolvency. But many larger banks opposed the law. Smaller banks and thrifts lobbied for it because they wanted protection from the competition of the larger, safer banks that were drawing away their customers.¹⁴ Protection was achieved by making all banks look equally safe to depositors. Risky banks and thrifts today form a similar lobby against risk-based premiums for deposit insurance.

Was Regulation Necessary?

At a price of \$3,200 per family, perhaps more, the possibility exists that a program of federal deposit guarantees has not been the most cost-effective way of giving depositors peace of mind. A straightforward policy of laissez faire and caveat emptor might have cost most people less. In an unregulated environment, Salsman suggests (82–87), banks would have taken steps to insulate themselves against the threat of bank runs by less costly means. They would have held more capital, acquired more diversified portfolios of higher-quality assets, avoided exposure to losses from interest-rate movements, retained more liquidity, and better diversified against geographical risks by branching more widely.

Salsman's conclusion—that government deposit guarantees and lender-of-last-resort policies were not necessary to stem bank runs—may be startling to some readers. But it is in fact well supported by studies of historical and foreign banking systems free of the guarantees and restrictions that currently govern U.S. banks. In such systems a run on an individual suspect bank was generally not contagious. In the absence of legal restrictions that weaken banks in similar ways, bank failures did not occur in droves, so depositors did not infer from one bank's difficulties that all others were about to default. No contagions are recorded in Canadian or Scottish banking history, where banks were free to branch nationwide and to capitalize adequately. Even in the United States there is little evidence (apart from the exceptional year of 1933) of runs spreading generally from insolvent to solvent banks.¹⁵

Salsman suggests that in the absence of government regulation, a fairly traditional type of banking would have prevailed. But without deposit guarantees, banks might have been led to restructure

their checking accounts to make them more run resistant, either by inserting a contractual clause giving the bank the option of requiring advance notice for withdrawals, or by linking checking to money-market mutual funds that cannot become insolvent. There are historical precedents for both alternatives.¹⁶ Mutual-fund checking is currently less prevalent than it might be for at least three reasons: (1) debt-based checking accounts receive subsidized deposit guarantees; (2) banks are not allowed to offer mutual funds; and (3) mutual fund firms are denied direct access to the Federal Reserve's interbank clearing system.

In the absence of government regulation and deposit guarantees, caveat emptor would undoubtedly be the watchword. With free entry into limited-liability, extended-liability, and mutual-fund banking, depositors who chose limited-liability banks would be choosing to expose themselves to default risk, to whatever extent a private insurance market would leave them uncovered, in exchange for whatever comparative benefits limited-liability banks could offer them. The problem of assessing the safety of a non-failure-proof bank might be handled by certification agencies, in the same way that private clearing-house associations have historically acted to certify the solvency and liquidity of their member banks.¹⁷

The solution of full deregulation is politically unlikely in the near future. Rather than repeal the regulations that systematically weaken U.S. banks, Congress, in the FDIC Improvement Act of 1991, chose to tinker at the margins, and even to add greater regulatory micromanagement of banks. The Act made some adjustments in deposit guarantees, but none that increased depositors' incentives to keep an eye on their banks. Congress balked at proposals to lower the deposit guarantee ceiling from the current \$100,000 per account, to limit the number of insured accounts per individual, or to leave 10 percent of each account's balance uncovered by the FDIC (possibly creating a market for private coinsurance). It refused to consider dismantling the legal barriers to interstate branch banking or the union of commercial and investment banking.

Before the advent of deposit insurance, depositors had strong incentives to move their funds from suspect to sound banks. Such movements were occurring in the early 1930s, and the private interest of weak banks in opposing such a "flight into quality" explains why small banks enthusiastically supported the formation of the

FDIC, while many large banks opposed it. The same sort of weak-bank interest today opposes coinsurance and other proposals that would give depositors any incentive to shun weak banks. The class of weak banks, as Salsman notes, today includes many of the largest as well as the smallest U.S. banks. They fear that the dismantling of deposit insurance and the rest of the regulatory regime might deliver on its advocates' claims: it might reimpose strong market discipline on the banking system.

NOTES

1. This kind of scapegoating has many precedents. For a critique, see Salsman's essay, "Bankers as Scapegoats for Government-Created Crises in U.S. History," in Lawrence H. White, ed., *The Crisis in American Banking* (New York: New York University Press, 1993).
2. Leading accounts include Edward J. Kane, *The Gathering Crisis in Federal Deposit Insurance* (Cambridge, Mass.: MIT Press, 1985); idem, *The S & L Insurance Mess: How Did It Happen?* (Washington, D.C.: Urban Institute Press, 1989); R. Dan Brumbaugh, Jr., *Thriffs under Siege: Restoring Order to American Banking* (Cambridge, Mass.: Ballinger, 1988); George J. Benston and George G. Kaufman, "Risks and Failures in Banking: Overview, History, and Evaluation," in George G. Kaufman and Roger C. Kormendi, eds., *Deregulating Financial Services: Public Policy in Flux* (Cambridge, Mass.: Ballinger, 1986); James R. Barth, R. Dan Brumbaugh, and Robert E. Litan, *The Banking Industry in Turmoil: A Report on the Condition of the U.S. Banking Industry and the Bank Insurance Fund* (Washington, D.C.: U.S. Government Printing Office, 1990); Lawrence J. White, *The S & L Debacle* (New York: Oxford University Press, 1991). Here and elsewhere I draw heavily on my survey of this research in "Why Is the U.S. Banking Industry in Trouble? Business Cycles, Loan Losses, and Deposit Insurance," in my *The Crisis in American Banking*. More detailed citations are provided there.
3. Why, then, didn't the economists predict the insolvencies of the FSLIC and FDIC before they happened? Actually, they did. Edward Kane in particular warned Congress well in advance that trouble was brewing, and titled his prophetic 1985 book *The Gathering Crisis in Federal Deposit Insurance*, n2 above. Congress simply chose to look the other way and to hope that good luck (favorable movements in interest rates; in the prices of real estate, farm products, and oil; and in developing countries' ability to repay) would erase the thrift and banking industries' red ink.
4. A thrift institution could legally have avoided the bet by funding its 30-year mortgage loans with 30-year certificates of deposit. But an institution pursuing this strategy would probably not have attracted enough funds to

survive. Its competitors would have priced it out of the market by taking advantage of deposit guarantees to raise funds more cheaply with short-term deposits.

5. L. H. White, 12-13.
6. For an application of this sort of cycle theory to the recession of 1991-1992, see William N. Butos, "The Recession and Austrian Cycle Theory: An Empirical Perspective," *CRITICAL REVIEW* 7, nos. 2-3 (Spring-Summer 1993).
7. Barth, Brumbaugh, and Litan, 81.
8. For the argument that restrictions on deposit brokerage are ill conceived, see L. J. White, 126-28.
9. Benston and Kaufman, 62.
10. Barth, Brumbaugh, and Litan, 29, Table 4.
11. John V. Duca and Mary M. McLaughlin, "Developments Affecting the Profitability of Commercial Banks," *Federal Reserve Bulletin* (July 1990): 488.
12. Salsman's argument implies that movements in the banking industry's capital/asset ratio should be predominantly associated with movements in the denominator (assets), with the numerator (capital) remaining relatively stable. For graphical evidence that this did not happen in the 1980s, see L. H. White, 15.
13. Edward J. Kane, "FIRREA: Financial Malpractice," *Durell Journal of Money and Banking* 2 (May 1990): 2-10.
14. Eugene Nelson White, *The Regulation and Reform of the American Banking System, 1900-1929* (Princeton: Princeton University Press, 1983), 196-97, 221-22. White points out that preservation of small banks also motivated deposit guarantee legislation at the state level: "No state that permitted branch banking seriously entertained the idea of deposit insurance."
15. George G. Kaufman, "Bank Runs: Causes, Benefits, and Costs," *Cato Journal* 7 (Winter 1988): 566-71; Anna J. Schwartz, "Bank Runs and Deposit Insurance Reform," *Cato Journal* 7 (Winter 1988): 591-93.
16. Lawrence H. White, "What Kinds of Monetary Institutions Would a Free Market Deliver?" *Cato Journal* 9 (Fall 1989): 367-91.
17. Each member bank in a clearing-house system, in order to reduce the default risk on the liabilities of its fellow members that it accepts daily, has such a strong incentive to agree that all clearing-house members must adhere to liquidity and solvency rules. Richard H. Timberlake, "The Central Banking Role of Clearinghouse Associations," *Journal of Money, Credit, and Banking* 16 (February 1984): 1-15; Lawrence H. White, *The Theory of Monetary Institutions*, unpublished book typescript in progress, ch. 4.

THE RATIONALIZATION OF CENTRAL BANKS

Charles Goodhart's The Evolution of Central Banks represents a rare and welcome attempt to spell out those shortcomings of "free banking" that supply a rationale for the establishment of central banks. However, one of Goodhart's principal arguments—that central banks are practically inevitable outgrowths of the "natural" tendency for bank reserves to become concentrated in a dominant "bankers' bank"—understates the role legal restrictions have played in sponsoring the emergence of bankers' banks, including the Bank of England. Some of Goodhart's other arguments against free banking rest on an exaggerated view of the likelihood of business cycles and financial crises under free banking as compared with central banking.

There has never been any shortage of criticisms of particular central banks and their policies. Moreover, central banking in general has recently been criticized as an undesirable and ultimately unnecessary intrusion of government into what some think would otherwise be well-working and stable capitalist monetary systems. What has been lacking in the economics literature has been any systematic effort to justify the presence of central banks by pointing to particular respects in which free banking—where numerous banks issue distinct brands of redeemable currency free from political regulation—would prove deficient.

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George Selgin, Department of Economics, University of Georgia, Athens, GA 30602, telephone (706) 542-1311, telefax (706) 542-3376, thanks Jeffrey Friedman, Richard Timberlake, Lawrence H. White, and three anonymous readers for their suggestions, while absolving them of the opinions and any errors contained in this review.

The shortage of scholarly arguments for central banking undoubtedly reflects the view of most economists that central banks are now a permanent feature of the economic landscape that no amount of debate will ever budge. In this regard I am reminded of a letter from a Federal Reserve spokesman, who assured me that the need for a central bank for the United States had been proven once and for all by a congressional vote in 1913.

To his credit, Charles Goodhart of the London School of Economics refuses to take the desirability of central banking for granted. Instead, he has shouldered the burden of showing how central banks came into being and why they are necessary. In the process Goodhart confronts many arguments for free banking. His resulting work on *The Evolution of Central Banks* (Cambridge, Mass.: MIT Press, 1988) represents, in consequence, an unusual attempt to spell out the failings of the free market in the realm of money.

The first chapters of Goodhart's book grew out of a set of working papers examining the arguments and drawbacks of free banking. Chapter 2 reviews traditional arguments for free banking that treat it as a wholly decentralized arrangement. Chapter 3 reviews the role of clearing houses, which may, according to more sophisticated arguments for free banking, provide a private (i.e., non-legislative) basis for regulation and centralization of reserves. Chapters 4 and 5 describe what Goodhart perceives to be inherent instabilities of free banking, including its tendency to fuel business cycles. In chapter 6, private deposit insurance is critically assessed as a means for preventing crises under free banking.

Goodhart's pro-central banking arguments are stated and recapitulated in chapters 7 and 8, which also draw on a 1987 article. Chapter 8 is followed by a lengthy appendix, reviewing the nineteenth-century record of central banking in Europe and Japan.

Goodhart is amply qualified to undertake his self-assigned challenge. Formerly an economist for the Bank of England, he is familiar with the details of actual central bank operations. At the same time, he is thoroughly acquainted with theoretical writings on money and banking, as is evident from his own frequently insightful contributions to this literature. Finally, Goodhart, unlike many other monetary economists, is both familiar with monetary history and aware of the crucial role institutions have played in that history. The result of these exceptional qualifications is a work for which all monetary economists should be grateful. Nevertheless,

this work offers no occasion for rejoicing on the part of central bankers of the world, for Goodhart's scholarly defense of central banks mainly succeeds in showing that the case to be made in favor of them is, after all, surprisingly weak.

The Case for Free Banking

The need for various kinds of legislative interference in banking, and for legislation establishing central banks in particular, has been taken for granted by most monetary writers at least since the passage of the English Bank Act of 1844. Peel's Act eventually guaranteed the Bank of England a complete monopoly of paper currency in Great Britain, effectively ending the monetary controversies of preceding decades with a verdict in favor of a (rule-bound) central bank. Yet a few stalwarts continued to insist upon the theoretical superiority of free banking, where numerous banks of issue are "regulated" by competitive pressures only. One of them was Walter Bagehot who, while editor of *The Economist* in 1873, published his highly influential book on *Lombard Street*.¹ Bagehot viewed free banking as an ideal that was both more stable and more "natural" than central banking. In contrast, he viewed the concentration of legal privileges in the Bank of England of his day as both unnatural and dangerous to economic stability. When it came to offering practical advice, however, Bagehot did not propose taking away the Bank's special privileges: that, he said, would be like proposing a "revolution," and just as fruitless. Instead, Bagehot hoped that the Bank could be persuaded to manage its affairs in a manner more conducive to the avoidance of financial crises (and, by implication, less conducive to maximizing the Bank's profits).²

In presenting his case for free banking, Goodhart draws heavily on Bagehot's particular vision of a free-banking system. Although Goodhart also refers to works by Benjamin Klein and Friedrich Hayek, these have to do with hypothetical arrangements involving competing private issuers of irredeemable fiat money, which are a far cry from free banking in its conventional and traditional (Bagehotian) meaning. In a traditional free-banking system, rival banks issue notes redeemable in some "outside" money, like gold, that none of them can create.

Bagehot's defense of free banking suffers, unfortunately, from

his unwarranted assumption that a laissez-faire regime would involve having (gold) bank reserves scattered evenly throughout the banking system. Goodhart is quick to point out the fallacy in this view: economic considerations, including simplification of inter-bank payments and a reduced overall need for reserves, would favor some centralization of reserves even under laissez faire. From this indisputable truth Goodhart draws the conclusion that centralization of reserves in the *Bank of England* was itself an inevitable and therefore "natural" feature of the late-nineteenth-century British monetary system. The (Bagehotian) case for free banking is thus seen to rest upon a false dichotomy of free versus centralized banking. In reality, Goodhart suggests (17), freedom itself sponsors a centralization of reserves in particular banks. Those "bankers' banks" then naturally evolve into noncompetitive central banks, charged with managing the total quantity of reserves (and, indirectly, money) in the public interest. Central banking thus appears to be a natural and perhaps inevitable development.

Is Central Banking Natural?

Goodhart's view of central banking as an inevitable development depends crucially on his belief that bank reserves will naturally tend to gather in and be managed by some *particular* bank. In support of this belief Goodhart refers to the views of economist Henry Thornton. Thornton, as interpreted by Goodhart, claimed at the outset of the nineteenth century that the centralization of reserves in the Bank of England resulted "from its superior credit (not just from the advantages endowed by legislation)" (19). Thornton's claim begs at least two questions. First, if "advantages endowed by legislation" are not crucial, why have there been so few examples of reserves being concentrated in banks not enjoying such special advantages? Reserves did not accumulate in particular banks in Scotland, Canada, China, Australia, Sweden, Ireland, or Switzerland until those countries passed laws favoring particular banks, usually by restricting other banks' ability to issue notes. The one well-documented instance of banks depositing most of their reserves in a single rival bank *not* enjoying any special legal privileges is the case of the Suffolk Bank of Boston, which held reserves for and redeemed notes of other New England banks between 1825 and 1858. Even this one

instance was, moreover, no reflection of laissez-faire policies: it probably would never have emerged had the laws allowed branch banking, thereby letting individual banks issue and redeem their own notes at numerous locations throughout New England.

Second, what, if not "advantages endowed by legislation," was the source of the Bank of England's "superior credit"? What allowed the Bank eventually to dominate all the other London banking firms, including many much older than it? Contrary to the impression conveyed by Thornton and Goodhart, the superior credit commanded by the Bank of England's notes, which caused other bankers to hold the notes in place of gold, can only be explained by referring to the Bank's enjoyment of legal privileges. After 1697 these privileges included a monopoly of government accounts and limited liability status (a status denied to other banks for another one and a half centuries). In 1709 the Bank obtained an official monopoly in joint-stock banking, meaning that all other banks were thereafter limited to having fewer than seven partners. (After 1826, the establishment of other joint stock banks of issue was allowed, but only outside a 65-mile radius from the center of London; after 1833, the establishment of joint-stock banks was allowed in London, but only on the condition that they cease issuing notes. Both of these relaxations of the Bank's special privileges left its monopoly of London paper currency essentially intact.) Private bankers' habit of treating Bank of England notes rather than gold as their primary reserve medium was reinforced during the Bank Restriction of 1797 to 1819, when the Bank was allowed to suspend gold payments. In short, had it not been for the Bank's special privileges, there is no reason to suppose that other banks would have been inclined to favor it by treating its notes as primary reserves and reissuing them, instead of pushing notes of their own, as happened everywhere where several banks of issue shared the same legal status.

Clearing Houses and Branch Banking

The point of these remarks is to suggest that it is not at all natural for any one bank, in a laissez-faire arrangement, to become the custodian of other banks' reserves. This happens only under regulated circumstances, and especially where the law favors a particular bank while restricting its competitors. How, then, would banks

in an unregulated system take advantage of economies associated with centralized reserves? They could do so, without paying tribute to any of their rivals, through a combination of branch banking and the establishment of private nonbank clearing houses. In a typical branch-banking arrangement, a number of large banks have branch offices nationwide. After branch offices exchange checks and notes locally, interbank debts are settled at the head offices in major financial centers. Reserves can thus be concentrated in the head offices, where the need for them is greatly reduced through the "netting out" of most local interbranch debts. Branching alone can often achieve all genuine economies from reserve centralization. Where further economies are desired, however, a group of rival banks can obtain them without elevating one of its members to superior status by establishing a private nonbank clearing house.³ Such clearing houses were an important feature of most nineteenth-century banking systems. They were especially important in the United States, where many state governments prohibited branch banking. Where laws did not favor particular banks, as they did in England after the Bank of England was established, banks relied for the most part on branch banking or private clearing houses, but *not* on any embryonic central bank or "bankers' bank," as means for economizing on reserves.

Although his chapter 2 summary of "The Case for Free Banking" suggests misleadingly that the only choice is between central banking and arrangements involving "no central reservoir of reserves" (25), in chapter 3 Goodhart does get around to discussing clearing houses and branch banking. He continues, however, to view bankers' banks as the evolutionary norm, treating systems relying on nonbank clearing houses and branch banking alone as exceptional mutants. Thus, in discussing private clearing houses, he focuses on the Suffolk arrangement, which did indeed involve the concentration of reserves and clearing facilities in a firm that was itself a bank. The Suffolk thus serves better than other private clearing-house arrangements in illustrating "centripetal forces" which, according to Goodhart, naturally allow a single bank to dominate a nation's monetary system. But what about the many other systems that relied entirely on branch banking and *nonbank* clearing houses to save on scarce reserves? Goodhart mentions Scottish and Canadian free-banking regimes only to dismiss them as "oligopolistic." But these examples are not so easily brushed aside for several rea-

sons. First, it is not obvious that the Scottish and Canadian systems were oligopolistic in any economically relevant sense. At one time there were 29 note-issuing banks in the Scottish system, with 19 remaining at the end of the free-banking era in 1845. At the peak of its free-banking era in 1875, Canada had 51 note-issuing banks. Although attempts were made to form price-fixing cartels in these and other free-banking systems, none of them appears to have been successful.⁴ Second, even if the charge of oligopoly stuck, it would still leave us to wonder whether oligopoly might itself be more “natural” than central banking. Finally, many other banking systems that were by no means oligopolistic, including that of the United States (excluding the Suffolk system), relied for the most part on nonbank clearing houses to centralize bank reserves. These instances suggest that Goodhart is not justified in viewing bankers’ banks as typical outgrowths of *laissez faire*.

Goodhart’s equally casual dismissal of branch banking, which he also links to oligopoly, is no more satisfactory. He upholds the nineteenth-century U.S. arrangement of unit banks joined by correspondent relationships as a more natural alternative (34–37). This account will, I think, strike most American experts as perverse: most of us tend, I think with ample reason, to regard our arrangement as being a quite unnatural outgrowth of legal barriers to branch banking, which have only recently been tumbling down. Had such barriers not been present throughout most of U.S. history, it is practically certain that we would have had fewer, larger banks, many with nationwide branches, and with a considerably reduced dependence upon either interbank deposits or clearing houses.

I have devoted so much attention to Goodhart’s claim that *laissez faire* would lead inevitably or naturally to the concentration of bank reserves in a private bankers’ bank because it undergirds his principal defense of central banks. According to Goodhart, a fundamental conflict of interest makes it unlikely that any unregulated bankers’ bank can be relied upon to manage the stock of bank reserves in a manner consistent with the public interest: rather than act as a lender of last resort during a crisis, a private bankers’ bank might prefer to see even its solvent rivals fail. This was, as Bagehot emphasized, a major defect of British monetary arrangements in the last half of the nineteenth century. The practical solution, to which Bagehot pointed the way, was to reform established bankers’ banks (like the Bank of England), ensuring that they would

serve the public interest as well as possible. Ultimately this would involve making them into full-fledged central banks, possessed with monopolies of paper currency but forbidden to compete with other banks in the business of making commercial loans. I would agree with both Bagehot and Goodhart that an unreformed bankers' bank is a dangerous thing. Nevertheless, because I am not at all convinced that bankers' banks are themselves natural developments, I do not see this fact as constituting an adequate rationalization of central banking. On the contrary, I see it as warranting the condemnation of laws that allowed bankers' banks to develop in the first place.

Fiscal Motives, Monetary Consequences

Of course, the merits of any particular monetary system cannot be inferred from the system's origins, natural or otherwise: even if central banking was not at all inevitable or natural it might still be better than free banking. The way in which particular central banks emerged historically does, nonetheless, yield important clues concerning their merits compared with free banking. Consider again the Bank of England. Established in 1694 for purely fiscal reasons (to pay for William III's war with France), the Bank was not intended to serve as an instrument of monetary policy: it could hardly have been, since there was no such thing as "monetary policy" at the time. Indeed, the Bank's founders gave no thought to any role it might eventually play in managing Britain's money supply. Goodhart acknowledges this fact. But he fails to address a related and equally relevant fact, namely, that the Bank's founders also did not consider or realize how special privileges embodied in the Bank's charter would, in strengthening the Bank itself, weaken other English banks and thereby make them quite *unnaturally* dependent upon the Bank of England's largesse for their very survival.

The free-bankers' view is this: by stripping privileges from other English banks and concentrating them in the Bank of England, the Act of 1694 and other legislation that followed ruined any prospect for the development of a healthy and self-regulating competitive English-banking industry. By securing the use of its own notes as other banks' reserves, the Bank gained unprecedented lending

power—power that allowed it to involve the entire English monetary system in inflationary episodes. These would eventually lead to an “external” drain of gold reserves to other countries, obliging the Bank to respond with a sudden contraction of credit. Tragically, the same legislation that allowed the Bank of England to expose the British financial system to previously unknown shocks also left its would-be rivals legally and economically emasculated: limited to six partners or less, they were undercapitalized and underdiversified. Their fragility was such that they were prone to succumb to economic shocks that would have left larger banks—like those that flourished freely at the time in Scotland—unscathed. It was this emasculation of the English “country” banks—a counterpart of the legislation that empowered the Bank of England—that ultimately made it appear desirable to have the Bank serve as a lender of last resort, the central pillar without which the English (and later the British) financial system would collapse.⁵

Just how strong would England’s banking system have become had Parliament not interfered with its development in 1694 and after? The experience of Scotland, where banking privileges were relatively widely and generously distributed until 1845 (when Peel’s Act was extended to it), is instructive. As Lawrence H. White has shown, the Scottish free-banking system did not seem to suffer at all from its lack of a central bank; indeed, the Scottish system appears to have outperformed its English counterpart in every crucial respect. Total per-capita deposit and note-holder losses from Scottish bank failures were fewer; Scottish bank crises and suspensions were less frequent; and the overall quality of Scottish bank services was superior.⁶

Goodhart, as I have noted, downplays the significance of Scotland’s success: he attributes this success not to the system’s relative lack of regulation, but to its reliance upon branch banking and its low ratio of gold reserves to monetary liabilities (52). Here again, it must be stressed that branch banking is not peculiar to “oligopolistic” arrangements (assuming Scotland’s was one of these) but has emerged in most settings where it has not been precluded by legal restrictions. It therefore makes no sense to suggest, as Goodhart does, that the Scottish system was strong because it had branch banking *rather than* because it was free. As for reserve ratios being low, this was not a sign of Scottish banks’ weakness or dependence on outside support but, on the contrary, a symbol of their extraor-

dinary efficiency and command of public confidence. That, at least, is the view expressed by Rondo Cameron, whose reserve-ratio statistics are cited by Goodhart.⁷

Besides the Scottish example, we now know of dozens of other instances of relatively free banking episodes. Most of these have only very recently been investigated, so that Goodhart could not refer to research concerning them when writing his book. The fact remains, nevertheless, that these systems also appear in general to have performed well, thereby posing an even greater challenge to arguments concerning the necessity and desirability of central banking.

Would Free Banks Overissue Currency?

Ultimately theory as well as empirical evidence must be weighed in assessing the need for central banks. Goodhart's own understanding of theory causes him to conclude that any banking system without a central bank must eventually display grave defects, including an inability to contain supplies of money and credit within economically desirable bounds. The free bankers' case hinges, Goodhart observes, on the ability of interbank "clearings"—routine requests for payment of each other's notes and checks—to correctly constrain competing issuers. Net payments must be settled in some scarce, outside reserve medium, such as gold or (as in Scotland) London bills of exchange. The clearing mechanism serves to discourage any individual bank in a free-banking system from overexpanding its balance sheet relative to its rivals, by confronting the overexpanding bank with net clearing debits that can eventually bankrupt it by exhausting its reserves. This constraint differs markedly from the situation under central banking, where central bank liabilities tend to displace outside assets as a reserve medium and means of settlement, rendering the central bank peculiarly immune from clearing-system-based limits on its lending and money-creating activities.

Goodhart thinks the clearing mechanism incapable of adequately constraining a free-banking system for two reasons. First, Goodhart argues, routine clearings under free banking will "tend to lead all banks to expand, or to contract, at a broadly similar rate" *without* limiting the rate of expansion for the system as a whole. Second, individual banks can defeat the clearing mechanism's abil-

ity to limit their expansion relative to that of rival banks "by making their own liabilities [including notes] more attractive" (30).

I think the first of these two arguments is mistaken, and the second irrelevant. Routine clearings *do* limit the overall extent of money creation in a free-banking system, by forcing banks to maintain some definite ratio of precautionary reserves of outside money relative to their note and deposit liabilities. This ratio will vary as the banking environment varies, but it is not arbitrary. The demand for precautionary reserves reflects random variations in net balances owed by individual banks at the end of a clearing session: even if a banker is certain that his bank will gain deposits equal to its losses from withdrawals over the course of some series of clearing sessions, the banker will want to have some safety cushion of reserves to protect his bank against the odds of being a net debtor to other banks at the conclusion of any *one* clearing session. The greater the total value of transactions faced by a bank in a typical clearing session, the greater will be its need for precautionary reserves, other things being equal. Since the volume of transactions depends on the volume of bank money in existence, no bank having limited reserves will want to issue more than a certain limited amount of money. This view of things is, incidentally, not original with proponents of free banking but is, rather, implied by standard theoretical writings on reserve demand. Goodhart's opposite view reflects a popular misconception which has, unfortunately, been propagated even by such luminaries as John Maynard Keynes and Chicago economist Lloyd Mints.

Goodhart's second argument must refer to a situation where (1) an individual bank submits to losses in order to temporarily increase its market share, or (2) one bank profitably increases its market share relative to others, or (3) the *overall* demand for bank money has increased, implying a general fall in the velocity (rate of circulation) of bank money. Case 1 contradicts the usual view that firms are profit maximizers. As for cases 2 and 3, neither implies any change in total spending or any threat to bank liquidity; therefore neither can be said to represent a threat to macroeconomic stability indicating a flaw of free banking. On the contrary, both of the latter situations could represent desirable (and predictable) manifestations of competitive efficiency.

The Uninformed Poor

Goodhart also fears that, under free banking, ordinary people — poor people especially — “will generally have neither the expertise nor the time to maintain a continuous assessment of the standing, riskiness, and reputation of the several alternative banks” (57). In consequence, market discipline will not serve to weed out inferior banks and reward superior ones. The contrary opinion of free bankers reflects, according to Goodhart, their erroneous assumption that information about banks in a free-banking system is “costless.”

There are several problems with this argument. First, the case for freedom in banking is no more dependent on the assumption that information about banks is costless than the case for freedom in, say, the automotive industry is dependent on the assumption that information about different brands of automobiles is costless. What the success of free banking requires is not costless information, but information that is sufficiently inexpensive to make its acquisition by prospective bank customers practical.

Organized asset markets may help to disseminate bank-specific information easily and cheaply. Goodhart recognizes this, observing that “developments in the conditions of individual banks” may be “quickly [and relatively costlessly] signaled in changes in their equity market shares” (65). However, Goodhart observes, “it would still be costly in time and effort for individuals to monitor and to seek to interpret changes in the market value of all the alternative banks” (66). The complaint here overlooks the possibility of bank customers referring to secondary-market prices, not of bank equity, but of bank *liabilities*: bank certificates of deposit are today also traded in secondary markets. Of even greater significance are the organized markets in bank notes that were present when such notes were issued on a competitive basis. Because bank notes do not typically bear interest, their secondary market “price” serves as a clear indicator of bank-specific risk. Normally, if all is well, a bank’s notes will trade at par or face value.⁸ If, however, the bank is judged to be in trouble by market experts, including private note brokers, other banks, and clearing-house authorities, the notes will trade at a discount. The limitation of note-issuing powers to central banks has thus served to cut off an important source of bank-specific information that would be available under free banking.

Also, and despite what Goodhart implies, it is not at all true that

bank customers, to protect themselves against getting stuck with bad bank money, must acquire information about each and every bank. They need only convince themselves that one bank is safe, and then agree to accept only checks and notes of other banks that are on the list of "bankable" (i.e., trustworthy) monies that their own bank offers to receive routinely on deposit at par. By this means the greater part of the burden of information acquisition is placed, not on individual consumers, but on the banks themselves. Bad banks are weeded from the system by having their notes refused by other banks, or by a nonbank clearing house.⁹

Secondary markets for bank liabilities and equity would not necessarily provide information on all banks: many smaller banks today are private companies; and some banks (small ones again, mainly) might not issue notes even under free banking. Such concerns lead Goodhart to observe that information about *some* banks must necessarily be limited or very costly to acquire in a free-market setting (66–68). The observation is, of course, correct, but its relevance is unclear. As long as the market informs agents of the existence of some safe banks, continued patronage of other banks can only signify either customers' risk-loving behavior or their possession of information not available through markets. I see no reason why a healthy banking system should not be able to cater to such customers so long as it also can meet the needs of less financially sophisticated ones.

To suggest that the public could have adequate information concerning the soundness of banks in a free-banking system is not to deny that unsound banks have ever existed. It is to suggest, rather, that legislated regulations and restrictions are typically to blame for past information problems. A case in point is the circulation of poor-quality country bank notes in early-nineteenth-century England, complained about by Henry Thornton in a passage quoted by Goodhart. Goodhart overlooks the fact that the survival of such poor-quality notes, that is, of notes issued by undercapitalized and underdiversified banks, was a direct consequence of the previously mentioned laws restricting banks, other than the Bank of England, to six partners or less. This legal restriction of competition gave English consumers little choice but to accept notes of little-known country banks, the Bank of England having refused (until well after Thornton wrote) to extend its own business into the countryside. Had English banking enjoyed the same freedom as Scottish bank-

ing did at the time, there would undoubtedly have been, not hundreds of small private English banks of issue, but perhaps a few dozen much larger joint-stock banks with nationwide branches and direct access to the London money market. In that case, the English public would have had little trouble locating and discriminating in favor of relatively "sound" bank monies.

The "information problem" that undermined the quality of bank-note currency in the United States prior to the Civil War was likewise a direct result of government interference. This time it was not a six-partner rule but barriers to branch banking that led to the proliferation of undercapitalized and geographically isolated banks of issue. Today in the United States yet another information problem stemming from regulation afflicts the banking industry. The problem here has not been that information costs are excessively high but rather that bank customers have lacked the incentive to acquire any information at all concerning bank safety. The cause of this is deposit insurance, which protects bank customers from losses they might otherwise suffer in patronizing risky banks. It is useful, by the way, to contrast the present U.S. banking situation with that in the still relatively unregulated and uninsured mutual fund industry, where information on the performance of individual funds is abundant (and is routinely sought by investors, who sometimes pay a high price for it). While it is true, as Goodhart notes, that mutual funds invest mainly in marketable securities whose riskiness and market value are easier to ascertain than that of bank loans, it is also true that banks could (if they had any incentive to do so) strengthen their own portfolios with marketable securities, collateralized loans, and capital to win the confidence of risk-averse consumers.

It is also sometimes forgotten that bank customers are not risk-minimizers but seek a certain balance of risk and (expected) return. Indeed, lower-income depositors are often most willing to accept high risks, presumably for the same reason that the poor buy a disproportionate share of lottery tickets. In Hong Kong, where bank deposits are still uninsured, poor persons often prefer to keep deposits at relatively risky "native" banks, even though they could keep their money at one of several large and apparently safer "foreign-style" banks, including the Hong Kong and Shanghai Bank. The reason cannot be ignorance, since the relative safety of the foreign banks has been made abundantly clear over the years. It

is simply that the clients of native banks are, like it or not, more interested in earning potentially high rates of return than in protecting their investments from default risk. Incredibly, "experts" from the Bank of England cannot see this, and so have been trying to foist on Hong Kong a government deposit-insurance scheme like the one that has wrecked the U.S. banking industry.

The Contagion Bogey

Still another worry expressed by Goodhart about free banking has to do with the possibility that bank failures will lead people to hoard currency, "thereby precipitating a general run on bank reserves" (61). This belief is crucial to the traditional case for central banks acting as the lenders of last resort, as it points to the central bank as the only potential source of funds to quell a panic. If, instead of hoarding currency, customers of failed or failing banks merely switched to holding notes or deposits of other (competitive) banks, individual bank failures might not be a problem calling for government intervention.

It seems to me, though, that the only basis for the widespread belief that individual bank failures and runs are bound to spread, contagion-like, throughout a banking system is repeated incantations of the belief by central bankers and their defenders. In fact, empirical evidence of bank-run contagion effects is surprisingly thin—too thin, I believe, to justify the creation of such dangerous institutions as central banks have turned out to be.¹⁰ The theoretical basis for the belief in contagion effects is weak as well. In this connection Goodhart, quoting John Kareken, refers to a well-known article by Douglas Diamond and Philip Dybvig as the one "rigorous and detailed argument" for the existence of a bank contagion.¹¹ But to really understand the Diamond and Dybvig model (no easy task, even for economists) is to appreciate how it serves to justify central banking only in the eyes of those who already are convinced that central banks are necessary, but who are intellectually honest enough (as Goodhart obviously is) to worry about a lack of "rigorous" theorizing to back up their case. Suffice it to say that in the Diamond and Dybvig model (1) banks and producing firms are one and the same; (2) banks do not make loans; (3) goods are available only through banks; (4) there is no money; (5) many

bank “depositors” are residual claimants, that is, they are not promised a fixed return on their deposits; and (6) bank panics are random events characterized by shortages of *goods*. Relaxing only a few of these patently ad hoc assumptions in the direction of greater realism—for example, by introducing bank capital—overturns the model’s anti-free-banking implications.¹² One is forced to wonder whether *any* meaningful policy implications can be drawn from such a farfetched model.

The Borrowers

Goodhart, however, does not rest his case for central banking solely on the usual contagion-effect argument. Indeed, his repetition of that argument in chapter 5 is followed in chapter 7 by his admission that recent bank runs “have *not* involved an attempt by the public to move . . . into cash but have merely produced a flight of depositors from banks seen as excessively dangerous to some alternative placement (not cash)” (96). Goodhart recognizes, furthermore, that the ongoing substitution of run-proof, equity-based mutual funds for conventional bank deposits makes the contagion-effect argument less relevant than ever. Awareness of such facts might be expected to imply rejection of the whole lender-of-last-resort rationale for central banks. But Goodhart disagrees, arguing that even large-scale interbank movements of deposits would be extremely damaging to bank *borrowers*, and so “would require a continuing support role for a Central Bank to prevent and, if necessary, to recycle such flows” (97).

This argument in defense of central banking, though novel, is to me unconvincing, lacking as it does any reference to the kinds of problems usually thought necessary to establish the existence of a market failure and consequent need for action by the government. Despite his reference to effects of bank failures that “could be extremely damaging for the economy,” Goodhart’s appeal here is, not to any truly *macroeconomic* (i.e., economywide) consequences of bank failures, nor even to some limited third-party (“external”) consequences, but to mere *second*-party consequences, which point to no market failure of any kind. This does not mean that such consequences are unimportant: depositors may suffer a great deal if their (uninsured) bank fails. Also, as Goodhart stresses, a failed

bank's investment in information and goodwill with regard to particular borrowers may be lost, making it harder for those borrowers to secure credit elsewhere. But to allow a central bank to rescue failing commercial banks solely in order to avoid such second-party losses is a cure worse than the disease: like deposit insurance, Goodhart's preferred central bank strategy undermines normal incentives for bank customers to "shop around" for better banks or to establish credit with several (rather than just one), encouraging them to focus instead on obtaining the highest possible returns and the cheapest loans, regardless of risk. In short, if it were indeed the case, as Goodhart claims, that high information costs alone sometimes lead bank customers to deal with overly risky banks, Goodhart's approach would exacerbate rather than mitigate the problem. All things considered, the quality of banks would be better, and the extent of customer losses smaller, if central banks did not offer to rescue clients of failed banks.

Goodhart believes that central banks could aid failing banks without subsidizing bad ones by making use of their superior knowledge to "penalize inadequate and improper managerial behavior" (101-2). The problem with this argument is that it would have central banks "punish" bad banks only once they became so bad that they failed; otherwise the system would still tend to punish *good* banks while *rewarding* bad ones. Because it does not seem reasonable to impose special punishments (apart from those normally associated with failure) on managers who are merely "inadequate," and because the line between "inadequate" and "improper" is not easily drawn, it appears likely that the overall effect of Goodhart's arrangement (considered as a substitute for *laissez faire*) would still be to encourage rather than discourage bad banking. In short, the price we would have to pay to avoid mere second-party losses from bank failures would still be too high, although it might be less than it is under our present system of deposit insurance.

Central Banking in Europe

The last part of Goodhart's book—more than a third of the total—consists of an appendix reviewing developments leading to the establishment of European central banks. The appendix relies almost entirely on information from volumes issued by the U.S.

National Monetary Commission following the financial crisis of 1907. It is, indeed, largely a synopsis, with some critical comments, of the commission's findings.

The danger of relying on a single source—or group of sources sponsored by a single governing committee—is, of course, that the source may be biased in favor of a particular point of view. Although Goodhart writes that the National Monetary Commission was set up “to consider . . . the need for Central Banks,” the fact is that the experts chosen to head the commission were already convinced of the necessity of a central bank for the United States well before they commissioned any studies. As James Livingston bluntly puts it, the commission's research stands as “a monument to the proposition that facts and values are indissoluble.”¹³

I do not mean to deny that there is much useful information contained in Commission volumes. Nevertheless the authors of those volumes were chosen, and information selected and presented to the public, with a particular reform program in mind. Therefore, to be really helpful in objectively reconsidering the case for central banking, the commission's findings should be read critically and together with information contained in other sources, especially more recent studies of European banking history. Among these I would include Vera Smith's important and recently reprinted study, which is cited in various places by Goodhart but only once (in passing) in his appendix.¹⁴ Other important studies, all of which to some extent contradict the pro-central bank conclusions reached by the National Monetary Commission, include those gathered by Rondo Cameron in *Banking in the Early Stages of Industrialization* and *Banking and Economic Development*.¹⁵ Finally, there is a very recent collection, edited by Kevin Dowd, of case studies favorable to free banking entitled *The Experience of Free Banking*.¹⁶ (This last, however, appeared too recently to have been available to Goodhart.) These surveys, together with studies of individual country experiences too numerous to list here but cited in the above works, present a far different view of the necessity of central banks than the one offered to the American public in 1910 and now redisplayed by Goodhart. In many cases the differences are so great that it is hard to believe that the new studies refer to some of the same historical episodes covered by the commission. For example, Goodhart, relying on a National Monetary Commission source, claims that the Franco-Prussian War of 1870 denied

Swiss banks of issue access to the lender-of-last-resort facilities of the Bank of France, forcing the Swiss banks to run for cover and restrict credit. The resulting crisis, he contends, "persuaded the Swiss that they needed a Central Bank of their own" (52). In contrast, Ernst Weber, writing in the Dowd volume, finds that both nominal and real issues of Swiss bank notes *rose* throughout the 1870s, and especially just following the outbreak of the war, when stable-value Swiss currency was demanded in France to replace depreciating French currency.¹⁷ If anything, the Franco-Prussian War appears, in Weber's study, to have given a boost to free banking in Switzerland.

One is tempted to wonder, indeed, what might have been the progress of banking in the United States after 1907 had the American public been presented with the findings contained in recent sources instead of, or at least in addition to, those offered by the National Monetary Commission. Perhaps the thousands of bank failures and violent contraction of the money stock in the 1930s, or the wholesale bankruptcy of much of the banking industry in the 1980s, might have been avoided.¹⁸

NOTES

1. Walter Bagehot, *Lombard Street* (London: Henry S. King, 1873).
2. Sir Robert Giffen, the economist and Bagehot's contemporary, held remarkably similar views. See "Fancy Monetary Standards" in Giffen's *Economic Inquiries and Studies* (London: George Bell and Sons, 1905), 177-86.
3. According to a nineteenth-century Canadian banking expert, banks with branch networks had little to gain from establishing clearing houses in cities with fewer than seven banks. See Kurt Schuler, "The World History of Free Banking," in Kevin Dowd, ed., *The Experience of Free Banking* (London: Routledge, 1992), 17.
4. See *ibid.*, 18-19.
5. See Vera Smith, *The Rationale of Central Banking* (Indianapolis: Liberty Press, 1990), ch. 2; and Kevin Dowd, "The Evolution of Central Banking in England, 1821-90," in Forest Capie and Geoffrey E. Wood, eds., *Unregulated Banking: Chaos or Order?* (London: Macmillan, 1991), 159-95.
6. Lawrence H. White, *Free Banking in Britain: Theory, Experience, and Debate, 1800-1845* (Cambridge: Cambridge University Press, 1984). Goodhart, along with several other writers, claims that Scottish banks no less than English ones ultimately relied on the Bank of England as a lender of last resort. In truth, prior to 1845 the Bank of England never offered to serve as a lender of last resort to Scottish bankers, who in any case seldom bor-

rowed from it. See Lawrence H. White, "Banking without a Central Bank: Scotland before 1844 as a 'Free Banking' System," in Capie and Wood, eds. 49-59.

Goodhart also claims that Canadian banks prior to World War I looked upon New York City banks as a source of emergency funds. This does not prove much, since the United States also had no central bank at the time. Moreover, Canadian banks were generally net *creditors* to New York City banks, just as Scottish banks were generally net creditors to London. It is only natural for banks legally isolated from important financial centers to lend to those centers through correspondents.

7. Rondo Cameron, *Banking in the Early Stages of Industrialization* (New York: Oxford University Press, 1967), 92, 98.
8. This assumes that the costs of redeeming the notes are trivial, as tended to be the case in banking systems (like the Scottish and Canadian systems of the nineteenth century) with well-formed branch and clearing-house networks.
9. The potential for private ("club") supervision of banks is recognized by Goodhart. However, because he believes that the club is likely to be dominated by a competitive bankers' bank, he concludes that private supervision will tend to be undermined by conflicts of interest.
10. See George Kaufman, "Bank Contagion: A Review of the Theory and Evidence," *Journal of Financial Services Research*, forthcoming.

The U.S. banking crisis of 1933, which seems to fit the general contagion scenario, actually involved a general run on the dollar, sparked by (justified) fears that it was about to be devaluated, rather than any wholesale loss of confidence in banks. See Barrie Wigmore, "Was the Bank Holiday of 1933 Caused by a Run on the Dollar?" *Journal of Economic History* 47, no. 3 (September, 1988): 739-55.

11. Douglas Diamond and Philip Dybvig, "Bank Runs, Deposit Insurance, and Liquidity," *Journal of Political Economy* 91, no. 3 (1983): 401-19.
12. See Kevin Dowd, "Models of Banking Instability: A Partial Review of the Literature," *Journal of Economic Surveys* 6, no. 2 (1992): 107-32.
13. James Livingston, *Origins of the Federal Reserve System: Money, Class, and Corporate Capitalism, 1890-1913* (Ithaca: Cornell University Press, 1986).
14. Smith, n5 above.
15. Cameron, n7 above.
16. Schuler, n3 above.
17. Ernst Juerg Weber, "Free Banking in Switzerland after the Liberal Revolutions in the Nineteenth Century," in Dowd, n3 above, 187-205.
18. For an argument blaming the banking crises of the 1930s and 1980s, among others, on regulations and central banks, see my "Legal Restrictions, Financial Weakening, and the Lender of Last Resort," *Cato Journal* 9, no. 2 (Fall 1989): 429-59.

ARE CENTRAL BANKS NECESSARY?

Two recent studies use history and theory to examine the likely consequences of eliminating government intervention in the provision of money. Such proposals would end the central bank monopoly over note issue and replace it with note issues by competing banks. Supervisory functions of central banks would be dispensed with. Accordingly, the proposals would free banks from all regulations on entry, disclosure, geographical limitations, and the products they may offer to customers. Monetary and banking arrangements would be left to the market. Unfortunately, the historical evidence does not speak as loudly in favor of free banking as its proponents claim.

The instability of prices and real economic activity since the mid-1960s has revived an interest in radical monetary reform the likes of which were last observed in the 1930s.¹ What distinguishes current monetary reform advocates is the emphasis they place on free banking as the vehicle for restructuring the monetary system. Free banking would undo the nineteenth-century centralization of the right of note issue in the hands of the monetary authority and remove central banks as discretionary managers of the banking system in which they operate as lenders of last resort. Free-banking proposals rest on the argument that macroeconomic instability is attributable to central bank actions.

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Anna J. Schwartz, a Distinguished Fellow of the American Economic Association, is a research associate of the National Bureau of Economic Research, 269 Mercer Street, New York, NY 10003, telephone (212) 995-3451, telefax (212) 995-4055.

Free-bank advocates rely on historical evidence to refute the traditional objection that competition in banking is an invitation to overissue. They deny that the business of banking is an exception to the rule that competition is the most efficient way of supplying goods and services. The one essential characteristic of a stable monetary system, they contend, is a requirement that the liabilities of competitive banks be convertible into a real asset, although they may differ on the designation of that asset.

The two components of the stock of money in a country are currency — in the United States, dollar bills and coins — that the central bank issues, and deposits that private-sector banks issue. In addition to currency, a central bank provides reserves to private-sector banks. The sum of currency outstanding and bank reserves is known as high-powered money, also as the monetary base, and as outside money (money provided by a source outside the private-sector banking system). Bank reserves in a fractional reserve banking system determine the amount of deposits private-sector banks can create. Bank deposits are known as inside money (money provided inside the private-sector banking system). Historically, central banking developed under a metallic standard. That standard required a central bank to convert into specie on demand the currency it issued. Currency was redeemable in another asset. Today, central banks are subject to no convertibility requirement. The currency they issue is not redeemable in another asset.

Almost two-thirds of David Glasner's *Free Banking and Monetary Reform* (Cambridge: Cambridge University Press, 1989) is devoted to the evolution of money from domestic animals to commodities, precious metals, and coinage until the emergence of the banking business. Why did the state at that juncture in history seek a monopoly over money? According to Glasner, this monopoly had nothing to do with a desire to improve the efficiency of the monetary system; it was instead desired as an instrument to raise revenue, particularly for wartime finance, to help the state protect its sovereignty.

What has weakened the national defense argument for the state monopoly over money in more recent times, in Glasner's view, are the formidable tax-collecting powers of the modern state. Taking the place of the national defense argument are economic claims, namely, that competitive banking is unstable, and that discretionary monetary policy stabilizes the economy. The rest of Glasner's

book counters these arguments, in the course of which he offers a review of the gold standard and its breakdown, the Great Depression, theories of money, the Keynesian revolution, and the monetarist counterrevolution.

Glasner argues that the deregulation of interest rates has resulted in the creation of many interest-bearing financial instruments that are indistinguishable from a narrow subset that once was defined as money. "A competitive monetary system has willy-nilly emerged" (179), but operates within the institutional framework of a noncompetitive system dominated by the government. Glasner's book gives a sense of features of current financial markets.

Kevin Dowd's *The State and the Monetary System* (New York: St. Martin's Press, 1989) is less diffuse than Glasner's study but lacks a foreground of the ongoing financial system. Dowd's study concentrates on establishing the case for competitive private-sector banks with no central bank in operation. Like the seminal work of Lawrence H. White on the Scottish free-banking experience before 1845, note issue is the focus of Dowd's analysis of a competitive banking system.² Dowd's case for the elimination of government regulation of note issue rests on two propositions: (1) The operation of an interbank clearing mechanism would check overissue by an individual bank and by the system as a whole, not only of notes but also of deposits, and (2) private issuers in a competitive system would have incentives to establish confidence in the value of the moneys they produce, which would limit the quantities they could safely emit.

In opposing government intervention in the provision of inside money by private-sector banking institutions, Dowd and Glasner share similar views. They diverge, however, on whether government would retain a role as an issuer of outside money in which inside money would be redeemable.

Glasner opts for convertibility of inside money into units of money (let us say dollars) that would be redeemable in whatever redemption asset had the same market value as the value of the amount of labor used in calculating a wage index. Glasner's scheme is designed to give the dollar constant purchasing power in terms of wages. Government would be responsible for maintaining the wage index, although Glasner does not discuss its derivation.

Dowd distinguishes between the medium of exchange, the medium of redemption, and the medium of account. Since the existing

medium of account (i.e., government outside money) is "intrinsically worthless" (97), he proposes the establishment of an expert commission to redefine the existing medium of account in terms of commodities and financial instruments so as to keep the price level stable. No restrictions would be imposed on the liabilities banks issue as the medium of exchange, but banks would specify in the contract with holders of their liabilities the medium of redemption, which could be left to the market to determine. Arbitrage would move the value of the medium of account to its equilibrium level should its price in terms of bank liabilities be too high or too low.

Both authors agree that bank runs and panics occur because of state interference in the monetary system. Sudden demands to convert deposits into notes could not be met under nineteenth-century conditions, since the possibility of increasing the supply of notes was limited by legal restrictions. Under free banking, competitive banks would have no problem in satisfying the increase in demand for notes.

Panic in the past was created by a demand to convert deposits into cash or cash into the medium of account, say, gold. In a fractional reserve system, not enough gold was available to satisfy everyone's desire to shift. In Glasner's view panic conditions need no longer be feared, but this is not because there is now a lender of last resort and deposit insurance. Rather, money-market mutual funds are now available as run-proof instruments. A holder of money-market shares has no fixed-dollar claim, only the value of the shares when cashed in. Banks could offer their own equity accounts (if they were no longer prohibited by the Glass-Steagall Act), or could make their deposits free from default risk by building up their capital (Glasner, 196-97). To avert panics, Dowd would revive the option clause, used in Scotland in the eighteenth century until banned, which allowed banks to defer redemption of their liabilities for a prespecified period but required them to pay interest for the period of deferment. Compulsory deposit insurance would be phased out, although a market-determined arrangement might be introduced by banks and their customers.

The Historical Evidence Regarding Clearing Houses

A prominent feature of the literature advocating free banking is the emphasis on the clearing house as an automatic mechanism that regulates the money issued by its members. Under the clearing mechanism a bank that increased its issues disproportionately to those of other banks would experience a drain of its reserves. Adverse clearings would run down the bank's balances at the clearing house and force it to contract. Hence the clearing mechanism serves to check individual bank overissue, whether of notes or deposits. Frequent clearing of notes and checks is important, the literature stresses. What is unsettled in theoretical analyses is whether the limits to expansion to which an individual bank is subject apply also to the banking system as a whole.³

Dowd cites the performance of the Scottish banking system before 1845, the First and Second Banks of the United States, the Suffolk Bank in Massachusetts, and clearing arrangements in Canada to demonstrate the ability of clearing houses to control the quantity of money. Is his confidence justified?

In accounting for the "outstanding success of the Scottish free banking system," Roland Vaubel mentions freedom of entry, absence of limited liability, and no limits on the size of banks.⁴ He does not refer to the operation of the clearing system. White, the modern proponent of the Scottish banking industry as the exemplar of banking competition, describes the clearing arrangements as "self-policing" in that clearing losses restrained an individual bank from expanding faster than the average. In discussing restraint on the system as a whole he stresses not clearing houses but the role of internal and external reserve drains due to the convertibility of notes into specie (1-22).

Both the First (1791-1811) and the Second (1816-1836) Banks of the United States promptly presented state bank notes for redemption, and served to that extent as clearing houses that regulated the currency. In their own operations, however, their performance was erratic, the First Bank apparently having extended permanent loans to individuals and banks, the Second Bank being deformed by mismanagement before 1819 and overexpansion in 1828-1831.⁵

A year after its founding in 1818 in Boston, the Suffolk Bank began to redeem country bank notes it purchased at a discount from merchants, individuals, and other banks. In 1825 it modified

the arrangement, banks making a permanent interest-free deposit at the Suffolk, and remitting additional funds to cover note redemptions, with the issuing banks' notes being redeemed at par.⁶ The Suffolk unquestionably checked overissue by individual banks, but its total annual redemptions during the period 1834-1857 show no evidence that systemic expansions were self-correcting. The figures of total redemptions rise from \$76 million in 1834 to \$376 million in 1857, rising and falling with every cyclical upturn and downturn in the period.⁷ This clearing house, therefore, did not preclude monetary instability.

The final commonly cited example of the clearing mechanism is that of Canada in the nineteenth century. Canada authorized chartered banks to open branches and to issue notes equal to their paid-up capital but legally required them to establish redemption agencies at the main city in each of the then seven provinces and elsewhere as determined by the treasury. The chartered banks exchanged notes daily at clearing houses in each of the larger cities and their branches exchanged them at other centers, balances being paid in Dominion notes, a government-issued currency, or in drafts on commercial centers. The notes circulated at par. Some Canadian banks failed in 1907, but unlike the panic that year in the United States, no restriction of payments took place in Canada despite a currency drain. Because of the 1907 experience, in 1908 Canada raised the limit on note issue during the crop-moving season. Was it the clearing arrangement rather than branch banking, competitive note issue, or convertibility that contributed most to the good record of Canadian banks before 1914? After that year the Department of Finance acted as a quasi-central bank and in 1935 the Bank of Canada was established.⁸

The Historical Evidence Regarding Competitive Banks

What free-bank advocates call into question is the classical view that unregulated competition in the supply of fiduciary money would lead to overissue, the destruction of its value, and an infinite price level. If different private money producers offered undistinguishable homogeneous monies, convertible into one another at fixed exchange rates, overissue would indeed be possible. For competitive production of fiduciary money, however, output differen-

tiated by brand names is essential so that consumers can acquire information about the quality of each producer's money. The brand name is not only information for the consumer but capital for the producer.⁹

When monies are differentiated by issuer and traded against each other according to flexible exchange rates, overissue is counterproductive. Under these conditions, overissue by a single producer might gain him a short-term profit but at a cost of disinvestment in his brand-name capital. Overissue would be at the expense of long-run profit by making a producer's money less acceptable to the market. A competitive money producer would create confidence in his product by limiting its issue.

This is the theoretical case for competitive money issue. We can examine three historical episodes often cited as examples of competitive banking to see how closely they match the theoretical paradigm. These episodes have been interpreted by some observers as representing flexible exchange rates between competing monies. In fact, however, these competing monies were all convertible on demand into a single outside currency that served as the medium of account. Rather than being instances of internal flexible rates, these were internal fixed-exchange rate monetary arrangements that arose to minimize money-changing and valuation costs (that is, transaction and information costs) of competitive independent monies. This is one respect in which the historical episodes fail to fit the theoretical case.

In general, a propaganda element pervades the literature on examples of competitive banks. It focuses on their performance as note issuers, in part reflecting the historical period when bank liabilities were predominantly in the form of notes. The main reason for this focus, however, is to convince the reader that the record of competing banks as note issuers in the past surpassed that of monopoly central bank note issuers and that competitive banking systems without central banks in today's economies would be superior to existing monetary arrangements.¹⁰

We need to check the performance of banks in the three episodes that are cited as historical examples of competitive banking: free banking in the United States, the Scottish free-banking era, and Swedish free banking. Were banks free of regulation? Did competition in fact prevail? If so, did it prevent overissue? Did it result in macroeconomic stability?

The first episode is free banking in the United States. So-called free-banking laws in New York (1838) and Michigan (1837) permitted free entry—a condition for a competitive system—but enforced capital requirements, specie reserve requirements, and state bond investments as security for note issues, all forms of regulation. Sixteen other states followed suit but in only eight were free banks established. Distinguishable bank notes circulated at varying discounts from par determined by their distance from the cities where they were presented for redemption. Merchants and brokers accepted the notes at these discounts.

For a long period historical studies portrayed free banks as guilty of financial irresponsibility. However, the reputation of free banks as wildcatters has been challenged in a number of recent studies,¹¹ which find the incidence of wildcatting exaggerated, the losses noteholders experienced minimal, and evidence of contagion limited. Some state free banks were successful; many were not, possibly because of capital losses on the bonds in their portfolios that reflected the instability of state finances, possibly because the banks' own behavior produced falling asset prices.¹²

It was not dissatisfaction with free banking that ended the experiment. It was the Civil War and the formation of a national banking system with notes backed by federal bonds and the enactment, effective July 1866, of a prohibitive tax on state bank notes.

The assumption that free banks lowered barriers to entry in the U.S. banking industry has been questioned, since faster growth of bank assets did not invariably occur in states after the introduction of free-banking laws than was true of the national or regional average.¹³

Moreover, the free-banking era did not coincide with economic stability in the United States. Hugh Rockoff concludes that the evidence in favor of adopting a free-banking system now is inconclusive. On the one hand, a bimetallic standard before the Civil War produced a steadier and slower rate of growth of the monetary base than has been true of recent U.S. experience with fiat money. On the other hand, the absence of a lender of last resort under free banking was a shortcoming, in his view, notwithstanding the free banks' ability to issue currency.¹⁴

If one turns to the episode of Scottish free banking, the claim is that it functioned safely and efficiently for over a century until the Bank Charter Act of 1844 and the Act of 1845 restricted Scottish

banking. How valid is the claim? The theory of flexible exchange rates among distinctive monies is not borne out by Scottish banknotes. The banknotes circulated at par with no variations in their market value reflecting differences in perceived quality.

Other features of the Scottish system have also been interpreted as deviations from a free-market system. One relates to bank owners' unlimited liability except for three limited liability chartered banks. It has been argued that unlimited liability may be a barrier to entry and a limit on the size of firms, since a benefit of limited liability is that it provides access to capital markets.¹⁵ The impact of unlimited liability on free banking as a competitive system is thus controversial.

Even the role of the note exchange system has aroused controversy, since free-bank advocates imply that all note issuers benefited from it and should therefore have voluntarily joined. But some did not, and the larger banks exercised market power to compel their membership.¹⁶

Three additional aspects of Scottish free banking have been subjects of debate. One is the relevance of a lower failure rate among Scottish than among English banks from 1809 to 1830, when it is not significantly lower measured from 1772 to 1830.¹⁷ What is in contrast here is English country banks, which until 1826 were limited to six or fewer partners, and Scottish banks subject to no such limitation. The prohibition of adequately capitalized and diversified joint-stock banks presumably was responsible for the English system's instability. Was the failure rate of Scottish free banks with no limits on the number of partners proof of greater stability of free banks than regulated banks, or merely a reflection of the importance of adequate capitalization?

A second debate has centered on the relation of the Scottish free banks to the London money market. White depicts the former as each holding its own specie reserves. But when public confidence declined, were they actually dependent for help, first on the Scottish chartered banks as lenders of last resort, and ultimately on the London money market and the Bank of England?¹⁸

A third question about Scottish free banking involves the absence of full convertibility of the note issues, even after the Act of 1765 denied the legality of option clauses and affirmed redeemability on demand. Was this aspect a weakness or strength of the system?¹⁹ If Scottish banks did not uniformly redeem their notes on demand,

even when the law denied them this alternative, does their resort to the option clause, which deferred redemption until a future date, suggest that they were not exemplary, contrary to the claims of free-bank advocates? In Dowd's view, option clauses are desirable as a response to panic conditions.

I share the assessment of the Scottish experience expressed by Charles W. Munn: "Insufficient research has been done to prove some of the points which have been claimed by some of the participants on both sides of the debate; and to transform an insufficiently researched eighteenth- and nineteenth-century historical topic into a policy prescription for the late twentieth century seems to me to be a highly questionable procedure."²⁰

A third case of competitive banking occurred in Sweden before 1897. The Riksbank, in existence since 1656, remained the only bank in Sweden in the 1820s, following the bankruptcies of private banks in the preceding decade. In 1824 a royal proclamation authorized privately owned and operated partnerships with unlimited liability, and in 1830 the first such note-issuing bank was chartered. Many others followed even though usury laws limited their access to funds. In the 1860s, usury laws were repealed and joint stock banks were authorized but denied the right of note issue. Private bank notes competed with Riksbank issues until 1897, when it obtained a monopoly on note issues. The Swedish banking system was stable, both types of private banks and the Riksbank itself never having been subject to runs or failures. The gold standard and unlimited liability of the private bank issuers, according to one scholar, promoted stable note issue policies.²¹

The performance of the banks in these cases cannot be evaluated as reflecting solely evidence of competitive behavior. The case studies were drawn from monetary systems of the past in which banks were regulated in a variety of ways, in which outside money of specie was supplemented by government-controlled fiat money elements, and in which an inside money convertibility requirement existed. It is not easy therefore to sort out the contribution of the propositions with respect to competitive behavior.

What Role for the State?

Glasner believes that a competitive breakthrough in banking thanks to financial innovation is in process. Not only are regulatory barriers collapsing, but the supply of monetary services is now largely competitive. That a deregulated, competitive banking system will be successful depends less for Glasner on the evidence provided by the historical cases he cites than on the financial innovation of money-market mutual funds that has immunized the system against the threat of instability. He does not, however, discuss whether money-market mutual funds will displace notes and deposits. Mutual funds usually hold only very short-term liquid assets, not the typical loans and investments of a bank with note and deposit liabilities.

Glasner's view, which I share, is that even a competitive banking system providing stable inside money is not sufficient to guarantee price level stability. Only if government-provided outside money into which inside money is convertible is stable will price level stability, banking stability, and economic stability be attainable. In addition, he opposes not only an activist monetary policy, but a monetarist rule for controlling the quantity of money, and restoration of the gold standard.²²

For Dowd, in contrast, historical evidence is at the heart of his argument for deregulated, competitive private issue of notes and deposits coupled with a convertibility requirement. His position is uncompromising. Its aim is to achieve a depoliticized monetary system by dismantling the machinery of state intervention in money and erecting barriers against would-be interventionists. An appropriate sequence of reforms in his view would begin with giving banks freedom to issue notes, followed by phasing out deposit insurance, abolishing restrictions on bank assets and liabilities, appointing a commission to redefine the medium of account, abolishing the central bank, and reorganizing government finances.²³

Dowd advises countries with written constitutions to constitutionally prohibit government interference in their monetary systems. He denies that government might need monetary powers for use in war, since it could always finance additional expenditures by raising taxes or issuing debt.

Both authors agree, however, that a prerequisite of good perfor-

mance by a banking system is a relatively stable monetary environment that only outside money arrangements can ensure. In such an environment, price level stability prevails.

Both sets of reform proposals leave open some questions. Their basic idea is that banks are not inherently unstable and therefore should be free to choose any assets yielding the highest return as they judge it and to pay whatever price for funds they deem reasonable, with minimum risk for noteholders, depositors, creditors. Yet the contemporary relevance of the historical banking experience on which Dowd relies heavily and Glasner less so is overdrawn. The case for competitive note issue is not the same as the case for the deregulation of banks. Neither author suggests that a set of restraints should be imposed on the conduct of owners and managers. That seems to me indispensable in a banking system in which the activities in which institutions engage in are no longer regulated.

The object of such restraints would be to make sure that any mistakes of owners and managers that wipe out capital and reserves would prompt the immediate closing or reorganization of their institution. Stockholders might be liable for an assessment equal to their equity, and managers would lose their jobs. Holders of bank liabilities under these arrangements would be fully protected, as net worth would not have turned negative (liabilities would not exceed the value of assets), should the marginal costs of products and services the institution offers the market turn out to exceed market prices. Whatever risks institutions were prepared to undertake, whether the activities were on or off balance sheets, the mistakes would be borne by the owners and managers, not by noteholders, depositors, and taxpayers.²⁴

Possible restraints that would safeguard against excessive risk taking might include capital requirements, marking assets to market (using market rather than book values of assets), provision of information to the public, and double liability for shareholders. A monitoring agency could in principle be privately organized, although the right to close down an institution would require legislative authorization. Whether quasi-private or governmental, the agency would function under a set of conditions unlike those that guide existing regulators, who are concerned with bank location or branching and the products and services that banks provide.

With the banks' funds secure, failures would not precipitate runs

or panics. If deposit insurance were maintained, it could be privately administered, with the aim of providing depositors and creditors in reorganized or closed institutions immediate access to the full value of their claims. Some would argue that a lender of last resort would still be needed to bail out insolvent insurance funds, but I disagree because there would be no need to draw on insurance funds if banks were closed before their net worth turned negative.

In such a banking system, self-regulation would prevail, in the sense that each institution would have an incentive to be prudent in acquiring assets and funding their acquisition, on penalty that foolhardy owners and managers would lose their equity and stake in the institution. They would have utmost scope for lending and investing without interference by government, subject only to an agency acting as judge of the institutions' performance. The agency's role would be to close or reorganize institutions that have been mismanaged before losses have been sustained by their liability holders.

The second question left open by the authors is the role of government as provider of outside money. Glasner's position is ambiguous. Does he have in mind government-created outside money that stabilizes a wage index, or does he propose to limit the role of government to announcing the wage index each month? Dowd assumes that central banks can be shut down. Is this realistic? Central banks are entrenched everywhere.

Both authors propose convertibility of inside money into an officially designated medium of account representing real values. Their standards of real value, though valid intellectual constructs, have no track record. It is doubtful that they would have the emotional appeal necessary to impart confidence in money and win popular support, as the gold standard did in the past.

Under existing monetary arrangements, to the extent that banks make contracts that stipulate fixed rates of future payments to others and from others, the consequences are arbitrary. The price level or inflation rate that prevails at the time banks make their contracts could well be higher or lower when the term of the contracts ends, making fulfillment of the contracts a gamble. This is so because banks have no way of knowing whether the price level or the inflation rate they expect will be realized. Each central bank currently operates a fiat money standard at its discretion, with

no commitment to maintain any particular price level or rate of inflation.

If there is little prospect that Glasner's or Dowd's standard will be adopted, an available alternative is to freeze outside money at the existing amount, as Milton Friedman has suggested.²⁵ Private depository institutions would be freed to issue bank notes. The money-creating powers of the central bank would cease. In this way the state monopoly on money could be replaced by a private, competitive money system, anchored by the frozen monetary base of outside money. Price stability would depend on prudent increases in inside money.

NOTES

1. Examples of piecemeal reform proposals not in the radical mode include: Bennett T. McCallum, *Monetary Economics: Theory and Policy* (New York: Macmillan, 1989), 339-47; Allan H. Meltzer, "Limits of Short-Run Stabilization Policy," *Economic Inquiry* 25, no. 1 (1987): 1-14; James L. Pierce, *The Future of Banking* (New Haven: Yale University Press, 1991), 112-26; George J. Benston, *The Separation of Commercial and Investment Banking: The Glass-Steagall Act Revisited and Reconsidered* (New York: Oxford University Press, 1990).
2. Lawrence H. White, *Free Banking in Britain: Theory, Experience, and Debate, 1800-1845* (Cambridge: Cambridge University Press, 1984).
3. See Vera C. Smith, *The Rationale of Central Banking and the Free Banking Alternative* (Indianapolis: Liberty Press, 1990), 178-84; White, 17; George A. Selgin, *The Theory of Free Banking: Money Supply under Competitive Note Issue* (Totowa, N.J.: Rowman & Littlefield, 1988), 80; Kevin Dowd, *The State and the Monetary System* (New York: St. Martin's Press, 1989), 44-48.
4. Roland Vaubel, "A History of Currency Competition," in Pascal Salin, ed., *Currency Competition and Monetary Union* (The Hague: Martinus Nijhoff, 1984), 61.
5. See the discussion of early American banking and the Jacksonian era in Larry Schweikart, "U.S. Commercial Banking: A Historiographical Survey" *Business History Review* 65 (1991): 607-18.
6. Donald J. Mullineaux, "Competitive Money and the Suffolk Bank System: A Contractual Perspective," *Southern Economic Journal* 53, no. 4 (1987): 884-98. Also see the discussion in Schweikart, 623-24.
7. Davis R. Dewey, *State Banking before the Civil War*, National Monetary Commission, 61st Cong., 2d Sess., Senate Doc. No. 581 (Washington, D.C.: Government Printing Office, 1910), 89.
8. Michael D. Bordo and Angela Redish, "Why Did the Bank of Canada Emerge in 1935?" *Journal of Economic History* 47, no. 2 (1987): 405-17. In

Canada, as in other countries with private-sector note issues, such as Sweden, the ratio of notes to deposits fell throughout the period before World War I, so that convertibility of notes per se was not as important as convertibility of total bank liabilities.

9. Benjamin Klein, "The Competitive Supply of Money," *Journal of Money, Credit, and Banking* 6, no. 1 (1974): 431-35.
10. A comparison of the monetary regime when bank liabilities were predominantly notes and the present monetary regime may be subject to the Lucas critique: the estimated parameters of the model would change in response to the regime change. See Robert E. Lucas, "Econometric Policy Evaluation: A Critique," *Carnegie-Rochester Conference Series on Public Policy* 1 (1976): 19-46.
11. Hugh Rockoff, *The Free Banking Era: A Reconsideration* (New York: Arno Press, 1975); Arthur J. Rolnick and Warren E. Weber, "New Evidence on the Free Banking Era," *American Economic Review* 73, no. 5 (1983): 1080-91; also idem, "The Causes of Free Bank Failures: A Detailed Examination," *Journal of Monetary Economics* 14, no. 3 (1984): 267-91, and "Explaining the Demand for Free Bank Notes," *Journal of Monetary Economics* 21, no. 1 (1988): 47-71. See also the references to free banking in Schweikart, 621-23.
12. Hugh Rockoff, "Lessons from the American Experience with Free Banking," in Forrest Capie and Geoffrey E. Wood, eds., *Unregulated Banking: Chaos or Order* (London: Macmillan, 1991), 73-109.
13. Kenneth Ng, "Free Banking Laws and Barriers to Entry in Banking, 1838-1860," *Journal of Economic History* 48 (December 1988): 877-89.
14. Rockoff, 103-5.
15. Jack Carr and Frank Mathewson, "Unlimited Liability as a Barrier to Entry," *Journal of Political Economy* 96, no. 4 (1988): 766-84. The argument has been countered on the ground that the chartered banks displayed no advantage in note issue or intermediation; they joined the note exchange system after the provincial banks had established it.
Lawrence H. White, "Banking without a Central Bank: Scotland before 1844 as a 'Free Banking' System," in Capie and Wood, eds., 37-62.
16. Charles W. Munn, Review of Lawrence H. White's *Free Banking in Britain*, *Business History* 27 (1985): 341-43.
17. See the table in White, 44, and the table in L. J. Sechrest, "White's Free-Banking Thesis: A Case of Mistaken Identity," *Review of Austrian Economics* 2 (1988): 251-52. Murray N. Rothbard finds low bank failure rates unacceptable as an indication of superior performance in "The Myth of Free Banking in Scotland," *Review of Austrian Economics* (1988): 230.
18. See Charles A. E. Goodhart, Review of Lawrence H. White's *Free Banking in Britain*, *Economica* 54 (1987): 129-31. Charles Munn reports that the archives note no cases of borrowing from the Bank of England by Scottish provincial and chartered banks. See his comment on White's chapter in Capie and Wood, eds., 64.
19. S. G. Checkland regards imperfect convertibility as a contribution to the

success of the Scottish banking experience, since it limited the banks' costs and dangers. See his *Scottish Banking: A History, 1695-1973* (Glasgow: Collins, 1975), 186.

20. Quoted in Capie and Wood, eds., 63.
21. Lars Jonung, "Swedish Experience under the Classical Gold Standard, 1873-1914," in Michael D. Bordo and Anna J. Schwartz, eds., *A Retrospective on the Classical Gold Standard, 1821-1931* (Chicago: University of Chicago Press, 1984), 361-99.
22. Glasner's own proposal for reform of outside money is somewhat murky. It is not clear whether a central bank has a role. Would inside money be convertible into government-created outside money that would stabilize a wage (rather than a commodity) index that Glasner espouses? Or would government simply announce the wage index that would minimize price-level uncertainty and have no further monetary function? In either case, his aim is a currency unit that would be indirectly convertible into the amount of purchasing power with a dollar value that would buy a certain amount of labor services.
23. Once the commission had announced the new medium of account and the date it would be effective, the currency unit would become convertible into the specified commodity bundle. Periodic revisions of the bundle would occur to maintain an unchanging value of the definition of the medium of account. Dowd would transfer the management of the government debt from the central bank to the state, obviously with the British arrangement in mind, suggesting that it would be possible to privatize its management. Government balances would be allocated among banks in accordance with their deposit market shares. The central bank would be shorn of any regulatory function, any responsibility for the currency, any participation in international organizations. Any remaining functions, such as managing clearings or payments, could be privatized or scrapped.
24. I regard bank failures as the kind of event that invites government intervention either to bail out management or to make bank depositors and creditors whole, using taxpayers funds. To preclude such government intervention, I favor closing institutions before their net worth has turned negative.
25. Milton Friedman, "Monetary Policy for the 1980s," in John H. Moore, ed. *To Promote Prosperity: U.S. Domestic Policy in the Mid-1980s* (Stanford: Hoover Institution, 1984), 40-52.

David L. Prychitko

AFTER DAVIDSON, WHO NEEDS
THE AUSTRIANS?
REPLY TO DAVIDSON

Paul Davidson asserts that Post Keynesians could fare just as well without insights from their Austrian colleagues. He's wrong. Radical subjectivists within both schools of thought have something to gain through dialogue, as evidenced by the efforts of Kenneth Boulding, G.L.S. Shackle, and Ludwig Lachmann. Many Austrian and Post Keynesian economists share a common methodological principle of radical subjectivism, which emphasizes nonergodic constructs and systems indeterminacy, and each school can gain from the insights of the other when asking such questions as why capitalism generates recessions and depressions and why capitalism generates any degree of order at all.

Consider a common phenomenon—unemployment. Why does it occur on a large scale in relatively free-market societies? Is it voluntary or involuntary? Is it the result of rational expectations or a cluster of errors or profound, intractable uncertainty? Do systemic market processes generate, and perhaps also help reduce unemployment, or can mass unemployment persist under some kind of unfortunate equilibrium state of its own? Why do relatively free-

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David L. Prychitko, Department of Economics, SUNY-Oswego, Oswego, NY 13126, telephone (315) 341-3417, telefax (315) 341-5444, is the author of *Marxism and Workers' Self-Management: The Essential Tension* (Greenwood, 1991), and co-editor (with Peter Boettke) of *The Market Process: Essays on Contemporary Austrian Economics* (Edward Elgar, forthcoming).

market systems generate, say, unemployment rates near 7 percent but rarely if ever generate 0 percent or 90 percent unemployment rates?

Can the standard neoclassical model of general economic equilibrium really account for and explain a world in continuous disequilibrium, a world where unemployment, profits and losses, and innovation and speculation abound? Post Keynesian economists don't think so. Nor do Austrians. If their indictment of standard theory is correct, surely Post Keynesians and Austrians can gain through an open dialogue with each other, for they, unlike standard general equilibrium theorists, are trying to explain systems characterized by unending disequilibrium conditions.

Paul Davidson disagrees. My purpose in this brief reply to Paul Davidson's wholly unsympathetic review ("The Economics of Ignorance or Ignorance of Economics?" *CRITICAL REVIEW* 3, nos. 3-4 [Summer-Fall 1989]: 467-87) of the O'Driscoll-Rizzo book¹ is narrow, and academic: I hope to try, perhaps vainly, to locate some common scholarly concerns between Austrian and Post Keynesian economics. I do not intend to provide a detailed defense of O'Driscoll and Rizzo against Davidson's criticisms; they are established economists who can defend themselves without my help. Nor do I intend to defend the bulk of Austrian economics against Post Keynesian objections. That would require a whole book, a project in which I have no particular interest. Consider this simply one Austrian's exploration of the problem, and reply. The narrow methodological issues at stake here (equilibrium versus disequilibrium, process versus end state, uncertainty versus ergodicity, convergent versus divergent expectations, etc.) ultimately shape our understanding of important theoretical and empirical issues—questions about economic order, unemployment, business cycles, and so on.

Equilibrium and Equilibrating Tendencies

One issue that seems to separate Austrians from Post Keynesians is the notion of equilibrium. Paul Davidson is unsatisfied with the O'Driscoll-Rizzo notion of "pattern equilibrium," a metaphor that smacks of ambiguity. So am I. Further, I believe Davidson is right on the mark when he points to the Arrow-Hahn-Debreu concept

of general equilibrium as being the most well-developed and clearly articulated notion of equilibrium in economics.² The Arrow-Hahn-Debreu equilibrium captures the Hayekian notion that there must prevail a mutual compatibility of plans,³ while also capturing the Misesian claim that general equilibrium spells the end of time, uncertainty, and human action.⁴ So, let's face it, while both Hayek and Mises understood quite early on the terribly unrealistic nature of general economic equilibrium, it is the great achievement of Arrow, Hahn, and Debreu to illustrate clearly and precisely what Mises and Hayek (and others) meant, by articulating just what conditions need to be met before a general economic equilibrium can occur.

Accordingly, with Davidson, I see little reason to redefine general equilibrium. Neoclassical economists should have their due: if they've given anything to the profession, it is a clear picture of what a society must look like if it is to enjoy being in general economic equilibrium.

Of course Davidson and I agree that that picture is appallingly fictitious. But so do all other Austrians. With remarkably few exceptions, we all live in a monetary economy. We act in a world of institutions and organizations, both intentionally and unintentionally created, that helps us cope with ignorance and uncertainty. Our world can be called anything but a world of general economic equilibrium. If our world is the "real world" of Oswego, Knoxville, or anyplace else, then equilibrium cannot, and will not, describe it. General equilibrium theory may, of course, be useful as a foil by which to understand why money, firms, and institutions exist in the first place. But it has no descriptive power, no empirical content.⁵

Most Austrians have traditionally argued, however, that rivalrous market processes are fundamentally *equilibrating*: unhampered market processes tend toward general equilibrium, but never quite reach it due to any number of exogenous changes. Which prompts Davidson's response:

The Arrow-Hahn results must cast considerable doubts on the Austrian presumption that there always exists the possibility of a plan-coordinated equilibrium that is just waiting to be discovered by entrepreneurs in a competitive market economy. Austrian subjectivists cannot have it both ways—they cannot argue for the importance of time, uncertainty, and money, and simultaneously presume that plan or pattern coordination must *exist* and is

merely waiting to be discovered. The very aspects of orthodox neoclassical analysis that the Austrians repudiate, its timelessness and absence of "genuine" uncertainty, are the guarantors of the existence of an equilibrium where individuals' plans are compatible.⁶

My disagreement with Davidson here is largely semantic. I do not believe the Austrian argument is that simple search costs are the major obstacle to equilibrium. Something important is lost in the interpretation between schools here.⁷ Nevertheless, I would agree with the spirit of Davidson's criticism: the Austrians have not shown how market processes have a fundamental convergence property without relying on assumptions that may threaten their allegiance to subjectivism and uncertainty.⁸ Austrian economists cannot, indeed, have it both ways. But I think they know that. I am not aware of any Austrian who has tried to demonstrate that the presence of "radical" subjectivism and "genuine" uncertainty is *compatible* with fundamental convergence properties toward general economic equilibrium. The argument is different. Some Austrians who have embraced radical subjectivism have criticized their colleagues for *ignoring* radical subjectivism. The merit of the O'Driscoll-Rizzo attempt is that they tried to discuss how market processes in the radically subjectivist world of profound ignorance can nevertheless lead to some degree of *order* (although not necessarily general economic equilibrium in the Arrow-Hahn-Debreu sense). They falter, however, in that they try to *redefine* equilibrium to meet their needs, to claim that an "equilibrium" may occur in which an ideal-typical pattern coordination results, in real time and uncertainty, rather than an exact plan coordination in the traditional sense. Calling it equilibrium invites the same kind of misunderstanding that Davidson's quote above falls into.⁹

*Ergodicity and Uncertainty: Neoclassical, Austrian,
and Radical Subjectivisms*

I agree with Davidson that neoclassical notions of "uncertainty" amount to nothing more than probabilistic "risk" (in Knight's sense of the term),¹⁰ fully captured in Samuelson's "ergodic hypothesis." Although risk is consistent with general equilibrium (and built into the Arrow-Hahn-Debreu contingency contracts), it does little to explain everyday market processes, the presence of money, com-

modity production, and so on.¹¹ There is, simply, no reason to hold money in a general equilibrium world. The question of monetary neutrality in a general equilibrium economy is somewhat dubious—money is nonexistent in general equilibrium.

Post Keynesians are interested in studying “nonergodic” processes, Davidson tells us. To the extent that a nonergodic world is one characterized by “uncertainty” (in Knight’s sense of the term),¹² there is no real disagreement between Post Keynesians and Austrians. Of course, to the extent that we move from Knightean uncertainty to the more radical Shacklean notion (which is, I think, the most fundamental notion of nonergodicity), it is true that most Austrians hesitate to accept nonergodic constructs.

This is the decisive rift, of course, between Ludwig Lachmann and his fellow Austrians. Lachmann fully embraced Shacklean uncertainty, and wished to merge it with Austrian subjectivism—to move from a theory of subjective tastes, preferences, and costs toward a more radical subjectivity of expectations.¹³ I fully agree with Davidson that, once we enter *this* world, we have no a priori basis on which to claim that markets are fundamentally equilibrating (in the standard sense of the term). It becomes an empirical question.¹⁴ Expectations may converge or diverge; moreover, convergent expectations are no guarantee of overall equilibrating tendencies, as one can easily show that even convergent expectations may lead to the disruption of others’ previously coordinated plans.¹⁵ For this reason, Lachmann argued that we simply do not know, based upon a priori reasoning alone, whether market processes are fundamentally equilibrating, and for this reason he met resistance from fellow Austrian economists.¹⁶ Lachmann is characterized as upholding an “equilibrium-never” position, polar opposite of the “equilibrium-always” dogma of rational-expectations economists. This leaves the Mises-Hayek-Kirzner story of the market process—as a fundamentally equilibrating process—on an allegedly comfortable middle ground between the two extremes.¹⁷

Teetering on the Middle Ground?

The intellectual tension between Lachmann and most other Austrians runs much the same as that between Davidson and the Austrians in general. The crux of the contention, or at least the method-

ological crux, as I see it, is radical subjectivism. I would hazard a guess that, when confronted by Post Keynesian and neoclassical criticisms, and teetering on the middle ground, some Austrians may tend to lean toward the equilibrium—always pole to keep from falling off, if only to keep subjectivism from going too far, and save economics from what they consider some form of vague institutionalism. In a way, Lachmann successfully imported some Post Keynesian subjectivism into the contemporary Austrian school. G.L.S. Shackle and Kenneth Boulding should also be given credit (or discredit, depending on one's perspective). But that may be giving too much credit to Post Keynesianism. After all, Lachmann, Shackle, and Boulding were all influenced very early on by Austrian school considerations of knowledge transmission processes in disequilibrated systems.¹⁸ Davidson's claim that "there is an obvious lack of communication between Austrians and Post Keynesians"¹⁹ lacks an appreciation of the historical development of the two schools of thought.

Research Programs, Research Questions

If my discussion so far has given Davidson or my Austrian colleagues the impression that contemporary Austrian economics will, or should, evolve into Post Keynesianism, let me offer the following caveat. I believe that radical subjectivism and the recognition of nonergodic processes may provide the communicative conduit between Austrians and Post Keynesians. Many of us speak the same language. Of course, the radical subjectivists within the Austrian school may find that they're more post- than neo-Austrian. Be that as it may, I think there's a case to be made for two-way communication. While we share that common methodological principle, we should also recognize, looking at the history of our two schools, that we are asking different questions about the same phenomena.²⁰ As Davidson states, "Keynes and the Post Keynesians developed different answers to the important questions that all reasoning people ask of economists, namely, What causes unemployment? What causes inflation? And how do we cure these maladies of the capitalist system without destroying capitalism?"²¹ These are urgent and difficult questions, and the efforts of careful scholars who pursue these questions should be supported and applauded.

But Austrians ask other questions such as, how can a highly decentralized, monetary economy, where knowledge is necessarily dispersed throughout society, show *any* degree of order? For example, how and why does anyone find successful, longer-term employment *at all* in an anarchic, rivalrous process? This way of raising questions is at least as important as the Post Keynesian approach. Perhaps even more important: the typical man on the street doesn't ask why employment occurs at all, because he already takes for granted that the capitalist system is supposed to work, is supposed to enhance job opportunities and a rising standard of living. The Austrian project, which differs from the Post Keynesian and neoclassical projects, is to explain what most of us simply take as given. I think Austrians can fruitfully pursue this project without relying upon the unduly restrictive and arbitrary assumptions regarding the nonneutrality of money,²² equilibrium or necessary equilibrating tendencies,²³ or a rational-expectations hypothesis.²⁴

The great merit of the O'Driscoll-Rizzo book lies in its suggestion that we get beyond the tired ideological divide that separates Austrians and Post Keynesians, and find that we have more in common at the methodological level than we once thought. O'Driscoll and Rizzo suggest that Austrians consider Post Keynesian insights more seriously. I suggest that Post Keynesians, too, follow through with a similar attempt at reasonable, open-minded dialogue. In this spirit perhaps subjectivists of both schools may have something to gain by asking questions regarding the ability of capitalism or any other system to generate order, and questions regarding the breakdown of order.

NOTES

1. Gerald P. O'Driscoll, Jr., and Mario Rizzo, *The Economics of Time and Ignorance* (New York: Basil Blackwell, 1985).
2. Paul Davidson, "The Economics of Ignorance or Ignorance of Economics?" *CRITICAL REVIEW* 3, nos. 3-4 (Summer-Fall 1989): 467-87, at 468.
3. See F. A. Hayek, "Economics and Knowledge," in idem, *Individualism and Economic Order* (Chicago: University of Chicago Press, 1980), 33-56.
4. See Ludwig von Mises, *Human Action* (Chicago: Henry Regnery, 1963), 244-50.
5. Leading neoclassical economists realize that general equilibrium theory has little, if any, empirical power. As Frank Hahn states:

General Equilibrium Theory is an abstract answer to an abstract and important question: Can a decentralized economy relying only on price signals for market information be orderly? The answer of General Equilibrium Theory is clear and definitive: One can describe such an economy with these properties. But this of course does not mean that any actual economy has been described. An important and interesting theoretical question has been answered and in the first instance that is all that has been done. This is a considerable intellectual development, but it is clear that for praxis a great deal more argument is required.

Frank Hahn, "General Equilibrium Theory," in Daniel Bell and Irving Kristol, eds., *The Crisis in Economic Theory* (New York: Basic Books, 1981), 123-38. Cf. Frank Hahn, *On the Notion of Equilibrium in Economics* (New York: Cambridge University Press, 1973).

In this light, Chicago school economics, which applies partial equilibrium models loaded with Tight Prior assumptions to interpret and describe empirical phenomena, is remarkably inconsistent. Jack Hirshleifer lets the general equilibrium cat out of the partial equilibrium bag when he states in his price theory text that "microeconomics concentrates mainly upon equilibrium states of particular markets, presuming an equilibrium of the market system as a whole" (Jack Hirshleifer, *Price Theory and Applications*, 2d ed. [Englewood Cliffs, N.J.: Prentice-Hall, 1980], 19). For a further criticism, see David L. Prychitko, "Rational Expectations and General Equilibrium," *CRITICAL REVIEW* 2, nos. 2-3 (Spring-Summer 1988): 220-24; and esp. Richard H. Fink, "General and Partial Equilibrium Theory in Bork's Antitrust Analysis," *Contemporary Policy Issues* 3 (Winter 1984-85): 12-20.

6. Davidson, 468.

7. Cf. Davidson (483n). In fact, Jack High's "Knowledge, Maximizing, and Conjecture: A Critical Analysis of Search Theory," *Journal of Post Keynesian Economics* 6, no. 2 (Winter 1983/84): 252-64, provides an Austrian criticism of search theory.

8. See, e.g., Jack High, *Maximizing, Action, and Market Adjustment: An Inquiry into the Theory of Economic Disequilibrium* (Munich: Philosophia Verlag, 1990), 70-82; and idem, "Equilibration and Disequilibrium in the Market Process," in Israel M. Kirzner, ed., *Subjectivism, Intelligibility and Economic Understanding*, 111-21; Tyler Cowen and Richard Fink, "Inconsistent Equilibrium Constructs: The Evenly Rotating Economies of Mises and Rothbard," *American Economic Review* 75, no. 4 (September 1985); Ludwig Lachmann, "On the Central Concept of Austrian Economics: Market Process," in Edwin G. Dolan, ed., *The Foundations of Modern Austrian Economics* (Kansas City: Sheed and Ward, 1976), 126-32; Peter Boettke, Steven Horwitz, and David L. Prychitko, "Beyond Equilibrium Economics: Reflections on the Uniqueness of the Austrian Tradition," *Market Process* 4, no. 2 (Fall 1986) (forthcoming in Peter J. Boettke and David L. Prychitko,

eds., *The Market Process: Essays on Contemporary Austrian Economics* (Aldershot, England: Edward Elgar).

9. But Davidson should know better. O'Driscoll and Rizzo spend some time carefully distinguishing among the standard neoclassical conception, the Hayekian conception, and their own notion of equilibrium (see O'Driscoll and Rizzo, ch. 5). O'Driscoll and Rizzo clearly are not discussing traditional, general economic equilibrium when they use their own pattern equilibrium construct. Hence their emphasis on uncertainty in equilibrium. From the Arrow-Hahn-Debreu definition that is, of course, oxymoronic. But so, too, is Davidson's own "unemployment equilibrium" analysis. If Davidson and other Post Keynesians can build a nonstandard equilibrium model, and therefore redefine equilibrium, why can't others?
10. Mises called it "class probability," 107-10.
11. Davidson, 477-82.
12. For Mises, "case probability," 110-15.
13. Ludwig M. Lachmann, "From Mises to Shackle: An Essay on Austrian Economics and the Kaleidic Society," *Journal of Economic Literature* 14 (March 1976): 54-62.
14. Interestingly, even Milton Friedman recognizes the equilibration question to be fundamentally empirical, and also seems to believe it is supported by experience. See Milton Friedman, "Comments on the Critics," in Robert J. Gordon, ed., *Milton Friedman's Monetary Framework: A Debate with His Critics* (Chicago: University of Chicago Press, 1974), 132-77, esp. 150.
15. See Christopher Torr, "Convergent and Divergent Expectations," in Kirzner, ed., n8 above, 295-300.
16. Israel Kirzner wonders whether the thrust of Lachmann's subjectivism is historicist. He unhappily suggests that "it would be a bizarre irony indeed if the modern revival of the Austrian tradition, begun over a century ago as a brave defense of economic theory (against a dominant tide of historicism), were to find its most sophisticated expression in the denial of any possibility of systematic market forces." See his review of *The Economics of Time and Ignorance* by O'Driscoll and Rizzo, in *Market Process* 3, no. 2 (Fall 1985): 2-3 (forthcoming in Boettke and Prychitko, eds., n8 above).
17. This is originally Roger Garrison's argument, which seems widely accepted among contemporary Austrians. See his "Austrian Economics as the Middle Ground: Comment on Loasby," in Israel M. Kirzner, ed., *Method, Process, and Austrian Economics: Essays in Honor of Ludwig von Mises* (Lexington, Mass.: Lexington Books, 1982), 131-38; and "From Lachmann to Lucas: on Institutions, Expectations, and Equilibrating Tendencies," in Kirzner, ed., *Subjectivism, Intelligibility, and Economic Understanding*, 87-101 (see n8 above). Garrison's argument resurfaces in "The Kaleidic World of Ludwig Lachmann," *CRITICAL REVIEW* 1, no. 3 (Summer 1987): 77-89. In the same issue of this journal, I had tried to support the thrust of Lachmann's radically subjectivist project, which perhaps explains why the tension was

advertised as "THE SUBJECTIVIST MAELSTROM" on the front cover of CRITICAL REVIEW.

Besides Garrison's original statements, Israel Kirzner provides the most thorough and compelling defense of the middle ground thesis in his *The Meaning of Market Process: Essays in the Development of Modern Austrian Economics* (New York: Routledge, 1992), ch. 1.

18. For the Austrian roots of Shackle's thought, see "An Interview with G.L.S. Shackle," *Austrian Economics Newsletter* 4, no. 1 (Spring 1983): 1, 5-7. Shackle's *Epistemics and Economics* (Cambridge: Cambridge University Press, 1972) has had a steady impact on contemporary Austrian economics, mainly through the efforts of Lachmann and Brian Loasby. See, e.g., Brian J. Loasby, "The Economics of Dispersed and Incomplete Information," in Kirzner, ed., *Method, Process, and Austrian Economics*, 111-30 (see 117 above). Boulding's "Austrianism" stems not only from his early work on capital theory, but his broad interest in the evolution of human knowledge. See, e.g., his *Image: Knowledge in Life and Society* (Ann Arbor: University of Michigan Press, 1956); idem, "The Economics of Knowledge and the Knowledge of Economics," *American Economic Review, Papers and Proceedings* 56, 1-13; and idem, *Evolutionary Economics* (Beverly Hills: Sage, 1981).
19. Davidson, 472.
20. See Axel Leijonhufvud's "Schools, 'Revolutions,' and Research Programs in Economic Theory," in idem, *Information and Coordination: Essays in Macroeconomic Theory* (New York: Oxford University Press, 1981), 291-345, esp. 319-27.
21. Davidson, 472-73.
22. Ibid., 473.
23. Ibid., 474.
24. Ibid., 475.

PAUL DAVIDSON AND THE
AUSTRIANS: REPLY TO DAVIDSON

Paul Davidson's critique of O'Driscoll and Rizzo is based on an "official" philosophical position that turns on an opposition between knowledge and ignorance (in epistemology) and a corresponding opposition between ergodic and nonergodic processes (in ontology). But Davidson's substantive analysis reveals a very different "unofficial" position, based on "sensible expectations" and a realist ontology of enduring social structures. While O'Driscoll and Rizzo have the edge on Davidson in terms of their characterization of agents' beliefs, their ontology of event regularities is ultimately the same, and suffers from the same drawbacks, as Davidson's official ontology. The solution to some of these problems may lie in adopting an ontology similar to the one Davidson invokes in his unofficial capacity.

As someone who has long felt that the Post Keynesians and the Austrians do share some common ground, Paul Davidson's attack on O'Driscoll and Rizzo's *The Economics of Time and Ignorance* ("The Economics of Ignorance or Ignorance of Economics?" CRITICAL REVIEW 3, nos. 3-4 [Summer-Fall 1989]: 467-87) comes as something of a surprise. Of course there have always been ideological differences between the two camps. And all too often these differences have

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Jochen Runde, Doris Woodall Research Fellow in Economics, Girton College, Cambridge, CB3 0JG, United Kingdom, telephone (0223) 33-8904, would like to thank Jeffrey Friedman, Martin Dietrich, Geoff Harcourt, Clive Lawson, Tony Lawson, and participants in the Realism and Economic Analysis seminar (Cambridge) for their comments on earlier versions of this paper.

stood in the way of effective discourse. But the topic of the present debate, or at least one of the main ones, is how to incorporate time and uncertainty without their becoming destructive of economic analysis. Given that both Davidson and the modern Austrians have devoted considerable thought to this issue, one might reasonably have expected a more sympathetic meeting of minds.

When I first undertook to write this piece, my intention was simply to highlight some common themes in Post Keynesian and Austrian thinking. But it transpires that any success I may have had in doing so has been for reasons quite different from those I originally had in mind. The difficulty that arose is that Davidson adopts not one, but two positions on doing economics, an official one (which he uses as a stick with which to beat O'Driscoll and Rizzo) and an unofficial one (which he adopts in his own substantive arguments). I shall argue that on questions of epistemology, Davidson is unduly hard on O'Driscoll and Rizzo in terms of his official capacity, and not substantially different from them in terms of his unofficial capacity. On the level of their respective (ontological) claims about the world, on the other hand, the official Davidson and O'Driscoll and Rizzo are at one, both reducing the issue to the presence (or absence of) empirical regularities between events. I shall argue, however, that the ontological presuppositions implicit in Davidson's substantive arguments transcend certain difficulties that arise in both his own official account and that of O'Driscoll and Rizzo.

The influence of Roy Bhaskar, in what follows, will be apparent to anyone familiar with his writings.¹ So as not to confuse the issue, however, I shall for the most part stick to the terminology employed by Davidson and O'Driscoll and Rizzo. Nevertheless, the words "epistemology" and "ontology" feature prominently below, and it is as well that I fix their meanings now. By epistemology, I mean the theory of knowledge and/or truth. By ontology, I mean the theory of being, of some object. As the titles of his books indicate, Bhaskar is a philosophical realist and, as such, emphasizes the theory of the object of analysis (ontology) over the theory of our knowledge of that object (epistemology). Since the present debate is primarily concerned with questions of knowledge, however, I shall begin with the epistemological and work my way back to the ontological. I am not sure how Davidson and O'Driscoll and Rizzo would feel about having a realist perspective taken on their

work. Perhaps, as they are both fond of referring to the "real" world, they would not object too much.²

The Official and the Unofficial Paul Davidson

In a spate of recent contributions, Davidson sets out his position on science and knowledge.³ His emphasis in these papers is on questions of epistemology, although, as we shall see, his conclusions about the possibility of knowledge about future economic events and relationships are based on an ontological precondition. Broadly, Davidson's position rests on an opposition between two basic categories, knowledge, on the one hand, and an absence of knowledge or "unknowledge," on the other. He begins by sketching what he understands by knowledge and then proceeds to define uncertainty in terms of an absence of knowledge.

Davidson considers two sources of knowledge, which correspond to the doctrines of rationalism and empiricism in traditional epistemology. The first of these, which he describes as "based solely on logic," emphasizes the role of a priori reasoning in knowledge. Davidson is skeptical about the usefulness of deductive reasoning when this is pursued without any real regard to its empirical foundations. He seems to prefer the second form of knowledge, that which emphasizes the primary role of experience. When it comes to "possessing *knowledge in the real world*," he writes, we must have an empirical basis which is (usually) quantitative and "is described via statistical averages which have been calculated from past (and/or current) time series and/or cross-sectional data."⁴ Now the emphasis here is on the words "past" and "current." Davidson argues that (historical) experience translates into ("scientific") knowledge that can be used to predict future events and relationships only under special circumstances. The necessary condition is that the relevant events or relationships be governed by ergodic processes:

In an economic world governed entirely by ergodic processes . . . economic relationships among variables are timeless, or ahistorical in the sense that the future is merely a statistical reflection of the past. The historical dates when observations are collected do not affect the estimates of the statistical averages. The accumulation of past evidence regarding economic outcomes allows the calculation of statistical averages which can then be used to make statistically reliable statements regarding the mathematical risk or probabil-

ity of events occurring in the future. In other words, *in an ergodic environment, knowledge about the future involves projecting statistical averages based on past and/or current realizations to forthcoming events.*⁵

We are now in a position to consider Davidson's conception of uncertainty and, specifically, uncertainty about the future. I have already noted that he defines uncertainty as an absence of knowledge. Then, since knowledge about the future presupposes the property of ergodicity on his account, Davidson is led to define uncertainty in terms of an absence of this property:

Scientifically speaking . . . *uncertainty about the future* involves a nonergodic environment. In a nonergodic world, past observations cannot produce information regarding current and/or future events; current patterns provide no statistically credible estimate of future time and/or space averages. . . . In a nonergodic world, people "do not know what is going to happen and know they do not know what is going to happen. As in history!"⁶

Davidson's official position thus consists of an epistemological dichotomy between knowledge and uncertainty and a corresponding ontological opposition between ergodic and nonergodic processes. The basic presupposition appears to be an ontology of what Bhaskar calls empirical realism. In terms of this doctrine, the basic objects of the world are the objects of actual or possible experience, and the aim of science is to isolate and identify empirical regularities. Where such regularities obtain, where the world is ergodic in Davidson's terminology, "scientific" knowledge is possible.⁷ Otherwise, in "nonergodic situations," it is possible only to have (historical) knowledge of past and/or current events. With respect to the future, in nonergodic situations, uncertainty prevails.

The fact that Davidson should advocate a form of empirical realism is surprising in at least two respects. First, it is strange that so vigorous an opponent of orthodox economics should espouse a position that amounts to little more the received view in applied economics/econometrics.⁸ The main difference between Davidson and the orthodoxy, if I am right, is that he is far less inclined to assume that economic events are governed by ergodic processes. The second curious aspect of Davidson's official position, the one I shall concentrate on below, is that his substantive arguments are not couched in terms compatible with it. I shall now show why this is so.

Sensible Expectations

Most Post Keynesian and Austrian economists would agree that economic observations are invariably “generated in nonergodic circumstances.” But this surely cannot be taken to imply, as Davidson writes, that “whenever decision makers recognise that they face nonergodic conditions [they] are therefore ignorant regarding the future.”⁹ For this would mean that “whenever nonergodic circumstances are involved”—almost always, that is—economic deliberation and analysis would be of no use at all.¹⁰ If nonergodicity implies ignorance about the future, then people have *no* basis on which to adjudge some things as more or less uncertain than others. They will then not be able to discern any pattern of association between their actions and the consequences thereof and their “decisions” will literally be powerless.¹¹ In these circumstances, for example, people would have no more reason to hold money as a store of future options or to enter into contractual relations than to take any other course of action.

Davidson does not appear to see this problem, but inadvertently manages to avoid it by invoking the notion of “sensible” expectations and behavior:

Sensible decision makers believe they operate in a nonergodic environment; they “know” that rational expectations in a statistically nonpredictable world are not sensible. Yet, since production takes time, commitments have to be made today if anything is going to be produced for tomorrow. When civilized people think they face an uncertain (nonergodic) future, they try to form sensible expectations which rely on the existence of social institutions that have evolved (e.g. contracts and money) to permit humans to cope with the unknowable.¹²

Considered in isolation, this passage offers little to disagree with. The problem, however, is that Davidson’s “sensible expectations” have no place on either pole of his knowledge/uncertainty opposition. The beliefs that future contractual obligations will be met, for example, and even that the institution of forward contracts will exist in the future, are both arrived at in a “nonergodic environment” and therefore do not qualify as knowledge on Davidson’s official account.¹³ But neither, apparently, are such beliefs the mere stabs in the dark one would expect in the absence of any knowledge whatsoever. My point, then, is that Davidson’s official epistemology requires a third category, one of (fallible) probable knowl-

edge (perhaps along the lines of Keynes's *Treatise on Probability*) or relative knowledge (perhaps on the line proposed by Tony Lawson) where this is based on an ontology other than the ergodic/nonergodic dichotomy.¹⁴ Unofficially, of course, Davidson introduces a category of this kind with his conception of sensible expectations. In the next section I shall say something about the implicit ontology that permits him to do so. First, however, it is necessary to turn to O'Driscoll and Rizzo on the question of knowledge.

O'Driscoll and Rizzo

Although O'Driscoll and Rizzo have a good deal to say about knowledge, they nowhere appear to define it. They are somewhat more forthcoming on their usage of the term "uncertainty," however. It turns out that they invoke both a conception of absolute uncertainty, what they call "genuine uncertainty," and a conception of probable knowledge or belief. Unlike the official Davidson, then, O'Driscoll and Rizzo reserve a place for beliefs that fall between (absolute) knowledge and "unknowledge" on the epistemological spectrum.

"Genuine uncertainty," for O'Driscoll and Rizzo, involves an "open-ended set of possibilities."¹⁵ They seem to have in mind something like Shackle's references to "as yet unimagined possibilities," events or sequels to choices not even thought of at the time decisions are made. This is an important form of uncertainty, perhaps the most radical we can conceive. It also seems the form of uncertainty most deserving of the label "unknowledge."

Davidson criticizes the "genuine uncertainty" conception for not being able to describe the uncertainty that arises in his example. This example, it will be remembered, is one in which an agent is told about the toss of a two-sided coin that has either (a) two heads, (b) two tails, or (c) a head and a tail. Since all possible states of the world are known here, this situation is not, as Davidson points out, one of "genuine uncertainty" in O'Driscoll and Rizzo's terminology. Davidson's criticism nevertheless seems unduly harsh, since "genuine uncertainty" is far more radical than that captured in his example and is not a priori any less relevant for the economist (quite the reverse, I would have thought).¹⁶ Neither need mention

of “genuine uncertainty” lead to the “handwaving” Davidson fears, since it is clearly acknowledged by the orthodoxy and, moreover, as something that cannot be accommodated within the standard Savage/Bayesian learning framework.¹⁷

But “genuine uncertainty” is not the only form of uncertainty mentioned by O’Driscoll and Rizzo. Unlike the official Davidson, they recognize that it is necessary to assume that individuals “know something about the likelihood of unknown [*sic*] eventualities . . . because the contrary assumption would paralyze all action.”¹⁸ To this end they introduce a conception of probable knowledge or belief. Unfortunately they are not particularly explicit or clear about what form such knowledge might take. The most revealing passage is their reference to Popper’s propensity theory of probability, to rank or weight “various possibilities in terms of their likelihood.”¹⁹

The weights are *attempts* to perceive objective propensities (Popper, 1959). If we say that a great deal of what individuals will do is affected by objective propensities, then the requirements of meaningful decision-making are met. The huge number of possible consequences of different courses of action can be pared down by a weighting scheme. This scheme, in turn, is something that bears a relationship to the external world and is not merely self or social delusion. Thus, there must be a base of “loosely determined” behaviour. With respect to this base, although something completely unexpected could happen, it is not likely. And what is not completely unexpected is more or less likely to happen as specified by underlying objective propensities.²⁰

This appeal to what is essentially a realist account of probability is surprising, coming as it does from two avowed subjectivists. For reasons that will become evident below, the idea that probability may be concerned with an unobserved reality (Popper uses the term “generating mechanism”) and that it is only “the superficial effects of this reality which we can observe” is an attractive one.²¹ Unfortunately, however, after having made the connection, O’Driscoll and Rizzo revert to a conception of knowledge based on experienced regularities between events (see below).

In summary, O’Driscoll and Rizzo include a conception of (fallible) probable knowledge or belief, a category that cannot be accommodated in terms of Davidson’s official knowledge/uncertainty opposition. To this extent, and to the extent they take some steps toward developing this notion, O’Driscoll and Rizzo have the edge on Davidson. If, on the other hand, the comparison

is between O'Driscoll and Rizzo and the unofficial Davidson, there is little to choose between them. Both see the need for a conception of probable knowledge or belief which, while not infallible, maintains some grip on the external world. The question that arises, then, is what the world might have to be like in order for this to be possible. I shall now compare the (often implicit) answers Davidson and O'Driscoll and Rizzo give to this question.

Davidson's Ontology

As we have seen, Davidson's official ontology divides the world into events governed by ergodic processes and events that are not. But this dichotomy is not of much use to those who already accept that events in the social world rarely, if ever, display the property of ergodicity. What is required is some ontological elaboration *given* that the social world is open. In this section I shall focus on two possibilities emphasized by Bhaskar and Lawson, that although the world is open (1) it is characterized not only by change but also by relatively enduring structures; and, (2) it is stratified or layered, in the sense that the reasons for the phenomena identified at one level emerge from mechanisms and structures lying at "deeper" levels.

By way of illustration I shall use one of Davidson's favorite examples, the institution of forward contracts denominated in money terms. I shall take it for granted that few economists would dispute Davidson's implicit assumption that this institution exists independently of his analysis, and that, if it is to work in the way he describes, it must be relatively stable and enduring. The important and more contentious question is at what level this stability lies. I should like to suggest that, in general, the relatively enduring nature of social institutions is not to be found in regularities between events. Davidson appears to accept this. He himself points out that it need not be the case that future delivery is ensured every time a forward contract is entered into, even where the parties act in initial good faith. Instead, on the view I am proposing, the stability of social institutions lies in the relations, positions, and practices they embody, as well as in the mechanisms and structures that give rise to and govern these relations, positions, and practices.

By entering a contract, the parties concerned agree to place

themselves into a particular relationship with respect to each other. This relationship is embodied in a commitment to accept and adhere to certain rights and obligations which specify and/or delimit the actions covered by the agreement. To some extent, these rights and obligations will be specific to the agreement in question. But they will also depend on and be governed by a complex structure of more general rules, practices and conventions. In most situations, this structure presupposes and, to a greater or lesser extent, comprises the legal system. This system acts both to facilitate contracting activity (by providing an existing set of rules and practices—some codified, some not—to draw on) and to constrain it (by forbidding certain kinds of contracts and/or practices). An important aspect of the legal system, as Davidson points out, is that it vests certain authorities with the power to enforce contractual agreements.²² Our knowledge of these powers and how to achieve recourse to them, is what gives us rational grounds to believe (“sensibly expect”) that contractual agreements will tend to be met in the future and, if not, that the injured party will be entitled to some form of compensation.

Just as the institution of forward contracts is the product of various underlying structures and mechanisms, so forward contracts “lie behind” and govern other phenomena. Davidson’s account of the effects of money contracts on the functioning of the market economy provides a good example of this, as well as illustrating the difference between the intended and the unintended consequences of human agency. An important intended benefit of entering into contracts is to render the future more predictable: “The institution of forward money contracts where delivery and payment are specified at a future date is an institutional arrangement which permits agents to deal with, and control the outcomes of, an otherwise uncertain future.”²³ The institution of forward contracts also has unintended consequences. One such consequence, a positive one in this case, is that the web of contracts stretching into the future at any point in time lends stability to the system, making it a less formidable environment for entrepreneurial activity:

Long-lived forward contracts are the way a free-market economy, in an uncertain world, builds in institutional price and wage controls over time. In a nonergodic world, such explicit money contractual anchors for future

events are necessary conditions for encouraging entrepreneurs to carry out economic activities in a market economy.²⁴

The remarks in the preceding paragraphs are far from exhaustive and are intended merely to illustrate the approach I have in mind. I have gone somewhat beyond Davidson's account of forward contracts, but not, I think, by introducing anything incompatible with it. It will be evident, however, that the present approach differs sharply from Davidson's official view of science and knowledge: the more stable and enduring features of the social world are not identified with empirical regularities, but with social structures and the mechanisms that give rise to them. At the same time, the extremes of methodological individualism and methodological holism are avoided. Instead, the emphasis is on what may be called a transformational view of society: that, in acting, agents draw on various structures (social relations, rules, conventions, positions) and, in doing so, contribute to the reproduction of these structures (although sometimes in a changed way). Social structures thus not only permit and facilitate human agency, but presuppose it.²⁵

O'Driscoll and Rizzo's Ontology

One of the most stimulating aspects of the *Economics of Time and Ignorance* is its emphasis on the historical or "real" time dependence of economic activity. Early on in their book, O'Driscoll and Rizzo draw on arguments by Popper and Schick to show it is logically impossible for an individual to predict his or her future actions in real time.²⁶ Further on, they illustrate the endogeneity of uncertainty in social contexts with the Keynesian beauty contest and the Holmes-Moriarty story invoked by Oskar Morgenstern.²⁷ The purpose of these arguments is to establish the inescapability of uncertainty and ignorance in real time, thereby guaranteeing a continuous flow of opportunities or "gaps" for the acquisition of new knowledge. Since the entrepreneurial discovery process feeds on these opportunities, O'Driscoll and Rizzo see the passage of real time as having causal properties: "time is causally potent and creative. This implies that all economic processes must involve the transmission and growth of knowledge. . . . The growth of knowledge is the endogenous force that endlessly propels the system."²⁸ Perhaps Davidson would not object to this.²⁹ The idea that the

passage of time itself generates a continuous stream of novel learning/profit opportunities reinforces the view that, in general, economic events are not governed by ergodic processes.

The question presently at issue, however, is how this emphasis on uncertainty and ignorance can be made to square with purposeful action. O'Driscoll and Rizzo state the problem as follows:

While complete stability and predictability are incompatible with time, their total absence is incompatible with action. . . . A stable framework within which it is possible to predict consequences is a logical prerequisite for purposefulness. The solution to this paradox lies in the recognition of typical and unique aspects of future events.³⁰

This passage is significant, in my view, in that it displays both the strength and the weakness of their approach. Its strength lies in the recognition that human agency presupposes some enduring "framework" or structure, and in the distinction between typical and unique aspects of events. O'Driscoll and Rizzo call such stable frameworks "types," which are arrived at by "typification," the process of "extracting what stability and regularity there is in the flow of reality."

The problem with "typification," however, is that it itself is no more than an attempt to identify constant conjunctions of (aspects of) events. At bottom, the only difference between unique and typical aspects of events is that typical aspects will always be present, or be present in a stable proportion of cases:

The stability of types is not to be identified with those aspects that actually *have* been repeated in case after case. Rather, stability refers to aspects that are repeatable, in the sense that they are not affected by the passage of time. Consider, for example, the prediction that there will be a police patrol tonight. This amounts to a pattern or type prediction because we are quite able to conceive of a patrol as persisting in some unchanged overall pattern through time. The precise route they will take, what they will find, how they will react to a bank robbery, and so on constitute the (relatively) unique or time-dependent features of an event.³¹

The irony, then, is that the identification of types amounts to no more than an optimistic version of the empirical realism Davidson espouses in his official capacity.³² When they try to explain such regularities, O'Driscoll and Rizzo are driven to invoke natural laws and, in the social world, the idea that "habituations" and the use of rules of thumb give rise to regular patterns of behavior (along the

lines of Heiner)³³ and, more optimistically, that certain kinds of "creative entrepreneurship" will yield stable regularities.

The typification idea is open to a number of objections. The first is the Humean one, that instances in which we have experienced empirical regularities do not entail anything about instances we have not experienced. As O'Driscoll and Rizzo argue that the economy is an open system, moreover, they are cut off from appealing to closures, spontaneous or otherwise, that would guarantee empirical regularities.³⁴ Second, typification cannot explain the many things we know about social structures which are not based on our experience of them. Third, by collapsing the world into the experience of events, typification propagates the "flat" undifferentiated ontology that tends to accompany empiricism. By concentrating on the question of *how* we could have knowledge of social structures, it draws attention away from what we do know about social structures (and could know) in terms of their causes, tendencies, powers, and so on.

As before, I should like to suggest that these objections can be surmounted by a shift in emphasis away from the search for empirical regularities toward the social structures relevant to the phenomenon being investigated. Police officers, in the present example, are members of society that, by virtue of a body such as the state, are put into a certain position relative to the civilian population. Associated with this position are certain powers (of access, authority, arrest, and so on), duties (to protect life and property, apprehend criminals, and so on) and practices.³⁵ It is our knowledge of these powers and duties that allow us to arrive at what O'Driscoll and Rizzo call "pattern predictions" of certain more general features of police patrols, and yet still not be able to predict such things as the precise route they will take and what they will find. It seems plausible, moreover, that this kind of knowledge goes a long way toward facilitating much of the coordination of activities we perceive in the social world.³⁶

* * *

Post Keynesians and modern Austrians share a strong interest in the theme of uncertainty in economic analysis. It has often been pointed out that this theme carries the risk of degenerating into a complete and ultimately destructive brand of skepticism. Both

Davidson and O'Driscoll and Rizzo recognize this danger and, I have argued, there are commonalities in the ways they attempt to skirt it (although not always of the kind they may have wished or had in mind). In the process, I have outlined an approach in terms of which uncertainty about future events is quite compatible with (albeit fallible) knowledge about the mechanisms and structures that give rise to and govern such events. This can be achieved, broadly speaking, by placing less emphasis on event prediction and more emphasis on an ontology of relatively enduring social structure and relations. This approach fulfils the requirements of both Davidson and O'Driscoll and Rizzo, in so far as it can comfortably accommodate both human agency and the social structure it presupposes.

Davidson's attack on O'Driscoll and Rizzo is undermined by the fact that it is based on a view of science and knowledge that appears to have left unmarked his own approach to substantive questions. I have argued that the fact he ignores his own advice may not be such a bad thing. For his unofficial epistemology is not substantially different from, and his implicit ontology is superior to, that of O'Driscoll and Rizzo. Perhaps O'Driscoll and Rizzo would agree that, on this last count, there may be some profit in taking note, not of what Davidson preaches, but of what he practices.

NOTES

1. Roy Bhaskar, *A Realist Theory of Science* (Brighton: Harvester Press, 1978); idem, *Scientific Realism and Human Emancipation* (London: Verso, 1986); and idem, *Reclaiming Reality* (London: Verso, 1989). For applications of Bhaskar's ideas to economics, see Tony Lawson, "Abstraction, Tendencies and Stylised Facts: A Realist Approach to Economic Analysis," *Cambridge Journal of Economics* 13 (1989): 59–78; idem, "Realism and Instrumentalism in the Development of Econometrics," *Oxford Economic Papers* 41 (1989): 236–58; idem, "Realism, Closed Systems and Expectations," mimeo, Cambridge University, 1991; idem, "Realism, Closed Systems and Friedman," *Research in the History and Methodology of Economics* (forthcoming); and idem, "Realism, Philosophical," in Geoff Hodgson, Warren J. Samuels, and Marc Tool, eds. *Handbook of Evolutionary and Institutional Economics* (Aldershot: Edward Elgar, forthcoming).
2. See Sheila C. Dow, "Post-Keynesianism as Political Economy: A Methodological Discussion," *Review of Political Economy* 2 (1990): 345–58; and Uskali Mäki, "Scientific Realism and Austrian Explanation," *Review of*

Political Economy 2 (1990): 310-44, for alternative realist assessments of Post Keynesian and Austrian economics, respectively.

3. Paul Davidson, "Sensible Expectations and the Long-run Non-neutrality of Money," *Journal of Post Keynesian Economics* 10 (1987): 146-53; idem, "A Technical Definition of Uncertainty and the Long-run Non-neutrality of Money," *Cambridge Journal of Economics* 12 (1988): 329-37; idem, "The Economics of Ignorance or Ignorance of Economics?" *CRITICAL REVIEW* 3, nos 3-4 (Summer-Fall 1989): 467-87; and idem, "Is Probability Theory Relevant for Uncertainty? A Post Keynesian Perspective," *Journal of Economic Perspectives* 5 (1991): 129-43. The position sketched in Davidson's review of O'Driscoll and Rizzo is a restatement (often verbatim) of that developed in the earlier articles.
4. Davidson, "The Economics of Ignorance," 478.
5. Ibid. An ergodic process is an example of what Bhaskar calls a closed system, a system that displays an empirical regularity or constant conjunction of events. On the view I am proposing, the social world is open in the sense that social phenomena are the product of, and/or are governed by, a plurality of underlying structures and mechanisms. Constant conjunctions of events then occur only when one (or perhaps a combination) of these mechanisms are isolated from any countervailing tendencies, that is, usually under controlled experimental situations. Bhaskar's point is that such mechanisms are also operative outside of experimental situations, that is, when countervailing tendencies prevent empirical regularities (Bhaskar, *Scientific Realism*, 27-50). Scientific explanation is then possible (in terms of powers, dispositions, tendencies, and countervailing tendencies) even when constant conjunctions of events do not obtain.
6. Ibid., 479. The quotation is from John R. Hicks, *Economic Perspectives* (Oxford: Oxford University Press, 1977), vii.
7. "'Scientific' economic knowledge about the future relationship among observable economic variables is defined in terms of the probability distribution functions and statistical averages generated by ergodic processes" (Davidson, "The Economics of Ignorance," 477, *emph. in original*).
8. This may have to do with Davidson's desire to engage the orthodoxy on its own terms. In fact, in an earlier article, he sets out the position outlined above under the heading "The neoclassical view of science and knowledge" (Davidson, "Sensible Expectations," 146-53).
9. Davidson, "Is Probability Theory Relevant for Uncertainty?" 129-43 at 136.
10. On this point, Alan Coddington's critique of Keynesian fundamentalism (and Austrianism) hits the nail on the head. What Coddington fails to notice, however, is that although Keynes's account includes a notion of absolute uncertainty, it also includes a criterion ("evidential weight") on which judgments of logical probability may be graded in terms of the uncertainty that attaches to them. See Alan Coddington, "Deficient Foresight: A Troublesome Theme in Keynesian Economics," *American Economic Review* 72 (1982): 480-87; Jochen Runde, "Keynesian Uncertainty and the

- Weight of Arguments," *Economics and Philosophy* 6 (1990): 275-92; and idem, "Keynesian Uncertainty and the Instability of Beliefs," *Review of Political Economy* 3 (1991): 125-45.
11. George L. S. Shackle, *The Nature of Economic Thought* (Cambridge: Cambridge University Press, 1966), 74.
 12. This particular quotation is from Davidson, "Sensible Expectations," 149. The notion of "sensible" expectations and behavior is prominent in all of the articles listed in n3 above.
 13. It is significant that Davidson places the word "knows" between quotation marks when elaborating on sensible expectations, which suggests that he has a form of knowledge in mind that is different from his official conception of knowledge.
 14. J. M. Keynes, *The Collected Writings of J. M. Keynes*, vol. 8 (London: Macmillan, 1973); Tony Lawson, "The Relative/Absolute Nature of Knowledge and Economic Analysis," *Economic Journal* 97 (1987): 951-70. The introduction of this third category would avoid a further difficulty that arises on Davidson's account: by treating knowledge as one pole of a simple opposition it becomes impossible to distinguish between the epistemological status of different kinds of knowledge. I would go along with Davidson on the kind of truth and certainty guaranteed by sound a priori reasoning, for example, and ignore the question of whether or not the standard axioms of logic and rules of inference are themselves not to some extent arbitrary. But our knowledge of the "facts" of science or history is a different thing entirely, resting as it does on a shaky base of observation, perception, and inference. By adopting a conception of relative or probable knowledge, Davidson could accommodate this difference, namely, that knowledge based on experience is fallible, whereas the knowledge arrived at by sound reasoning (and assuming the truth of the premises or axioms) is not.
 15. Gerald P. O'Driscoll and Mario J. Rizzo, *The Economics of Time and Ignorance* (Oxford: Basil Blackwell, 1985), 66.
 16. In terms of Davidson's official conception of uncertainty, it is also not possible to discriminate between "genuine uncertainty" and the uncertainty represented in his example. See n14 above.
 17. See Frank H. Hahn, "Liquidity," Economic Theory Discussion Paper no. 129, Cambridge University, 24. Although I cannot speak for the Post Keynesians, I believe Keynes might recommend the assignment of equal probabilities in Davidson's example, by appeal to the principle of indifference. In his *Treatise on Probability*, Keynes discusses a similar example—"the two cases following of balls drawn from an urn. In each case we require the probability of drawing a white ball; in the first case we know the urn contains black and white in equal proportions; in the second case the proportion of each colour is unknown, and each ball is as likely to be black as white. It is evident that in either case the probability of drawing a white ball is $\frac{1}{2}$. . ." (82). The same example appears in Daniel Ellsberg's seminal

critique of Bayesian probability, where it is shown that situations of this kind give rise to choices from which it is impossible to infer unique subjective probabilities (Daniel Ellsberg, "Risk, Ambiguity and the Savage Axioms," *Quarterly Journal of Economics* 75 [1961]: 643-69). Keynes, on the other hand, would assign equal probabilities to drawing a black or a white ball from both of the urns, but still maintain that the two situations are not comparable because they differ in terms of evidential "weight." In Davidson's example, then, Keynes might recommend the assignment of equal probabilities, but that the agent would nevertheless not be indifferent to the situation Davidson describes, and to the situation in which the agent is faced with a toss of a coin he knows to be fair. The assumption orthodox Bayesians make, and the assumption Keynes is not prepared to make, is that a unique cardinal value can be assigned to every judgment of probability. See Jochen Runde, "Keynesian Uncertainty and Liquidity Preference," *Cambridge Journal of Economics*, forthcoming (1994).

18. O'Driscoll and Rizzo, 30.
19. O'Driscoll and Rizzo are careful to avoid the term "probability," perhaps because they believe it implies knowledge of cardinal values. Occasionally, however, they refer to "probability weights" (O'Driscoll and Rizzo, 64, 66, 77).
20. *Ibid.*, 31.
21. Karl Popper, "The Propensity Interpretation of Probability," *British Journal for the Philosophy of Science* 10 (1959): 25-42.
22. Davidson, "The Economics of Ignorance," 480-81.
23. *Ibid.*, 481.
24. *Ibid.*
25. See Bhaskar, *Reclaiming Reality*, ch. 5.
26. O'Driscoll and Rizzo, 25-26.
27. *Ibid.*, 72-74, 83-84.
28. *Ibid.*, 62. O'Driscoll and Rizzo's contribution may thus be regarded as that of extending Kirzner's conception of entrepreneurial "alertness" to already-existing profit opportunities into a dynamic context in which profit opportunities are endogenously generated. See Israel M. Kirzner, *Competition and Entrepreneurship* (Chicago: Chicago University Press, 1973); and *idem*, *Perception, Opportunity and Profit* (Chicago: Chicago University Press, 1979). Kirzner hints at some of the themes developed by O'Driscoll and Rizzo in a later paper: *idem*, "Uncertainty, Discovery and Human Action: A Study of the Entrepreneurial profile in the Misesian System," in *idem*, ed., *Method, Process and Austrian Economics: Essays in Honor of Ludwig von Mises* (Lexington, Mass: D. C. Heath and Company, 1982): 139-59; and see Jochen Runde, "Subjectivism, Psychology and the Modern Austrians," in Peter E. Earl, ed., *Psychological Economics: Developments, Tensions, Prospects* (Boston: Kluwer Academic Publishers, 1988): 101-120 at 112-14.
29. Davidson disagrees with the dogma that the Austrian discovery/coordination process is incompatible with long-term unemployment. But

so do some Austrians. See Ludwig, M. Lachmann, *Capital, Expectations, and the Market Process* (Kansas City: Sheed Andrews and McMeel, Inc., 1977): 267–86, 181–93 and esp. 191. Austrian unemployment is primarily structural and, as against Keynes, emphasizes the possibility that new investment in some forms of capital equipment may act as a substitute for labor rather than as a complement to it. Neither, it may be added, do Austrians “accept the neutrality of money” (Davidson, “The Economics of Ignorance,” 482), although here again the emphasis is on relative price changes and distortions of the capital structure. (*Ibid.*, 289–307).

30. O'Driscoll and Rizzo, 76.
31. *Ibid.*
32. I say “more optimistic” because although Davidson sometimes suggests that events in the social world may display empirical regularities “at least for short subperiods of calendar time” (Davidson, “Sensible Expectations,” 148), he usually takes the “real world” to be synonymous with a “nonergodic world” (Davidson, “The Economics of Ignorance,” 485, 110).
33. Ronald Heiner, “The Origin of Predictable Behaviour,” *American Economic Review* 73 (1983): 560–95.
34. O'Driscoll and Rizzo, 23–24.
35. The acquisition of knowledge about social relations, positions, and practices will be a hermeneutic endeavor to some extent, something that should find favor with modern Austrians. See Don Lavoie, ed., *Economics and Hermeneutics* (London: Routledge, 1990) and Ludwig M. Lachmann, *The Market as an Economic Process* (Oxford: Basil Blackwell, 1986). On the view I am proposing, however, hermeneutics is regarded as a complement to, rather than a substitute for, causal explanations in terms of tendencies, powers, dispositions, and the like. See Uskali Mäki, “Practical Syllogism, Entrepreneurship, and the Invisible Hand: A Critique of the Analytic Hermeneutics of G. H. von Wright,” in Lavoie, ed., 149–76; and Bhaskar, *Reclaiming Reality*, ch. 5.
36. O'Driscoll and Rizzo seek to modify Friedrich Hayek's conception of equilibrium as the compatibility of individual plans by redefining it in terms of pattern coordination: “The plans of individuals are in pattern equilibrium if they are coordinated with respect to their typical features even if their unique aspects fail to mesh” (O'Driscoll and Rizzo, 85; the reference is to Friedrich A. Hayek, “Economics and Knowledge,” *Economica* 4 (1937): 33–54). It is evident that, any ambiguous prose aside, this idea does not imply “a vector of market prices which is consistent with market equilibrium” or that there will be “no tendency to change” (Davidson, “The Economics of Ignorance,” 468, 474–75). Rather, the idea is that the social world displays continuity as well as change, and that it is possible to form fairly accurate expectations of more general (typical) features of situations or phenomena while not being able to predict particular (unique) features.

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WHAT CAN AUSTRIAN
ECONOMISTS LEARN FROM THE
POST KEYNESIANS?
REPLY TO DAVIDSON

Paul Davidson has argued that there is little common ground between Post Keynesian analysis and the Austrian approach to economics. He claims that Post Keynesians (unlike Austrian economists) place great importance on the role of expectations and do not present a deterministic view of the economic system. In this essay the validity of such claims is examined.

Paul Davidson is the editor of the *Journal of Post Keynesian Economics*. In "The Economics of Ignorance or Ignorance of Economics?" his review of *The Economics of Time and Ignorance* by the Austrian economists Gerald O'Driscoll and Mario Rizzo (CRITICAL REVIEW 3, nos. 3-4 [Summer-Fall 1989]: 467-87), he finds little common ground between Austrians and Post Keynesians. If there is any common ground, some of it will surely be found in the writings of Ludwig Lachmann and George Shackle, neither of whom appears in Davidson's bibliography, although there is a reference to Shackle on page 477 of the review.

Jan Kregel, who is a member of the managing board of editors of the *Journal of Post Keynesian Economics*, has noted that Lachmann's

work "represents an important synthesis of Austrian and Keynesian elements."¹ Until his death in December 1990, Lachmann was one of the most prominent members of the Austrian school. He caused some consternation in Austrian circles by suggesting that Austrians had something to learn from the likes of Keynes, Shackle, and Davidson.²

At the time of his death, Shackle was a member of the Academic Board of the *Journal of Post Keynesian Economics*. Geoff Harcourt, who is also a member, has nevertheless pointed out that Shackle remains "an ardent admirer and friend" of Hayek, an Austrian economist.³ Indeed, Lachmann regarded Shackle as an Austrian economist.⁴

Nearly a decade ago, in an article to mark the centenary of Keynes's birth, Lachmann examined Keynes's thought from an Austrian perspective, and in the process presented a lucid account of areas of agreement and disagreement.⁵ Prior to this he made a point of bringing kaleidic concepts to the attention of Austrian economists.⁶ Davidson the Post Keynesian and Lachmann the Austrian thus have something in common: both have suggested that Austrians have something to learn from Keynes. Let us ask ourselves anew what Austrian economists can learn from the Post Keynesians.

In chapter 5 of the *General Theory*, Keynes employs the word "expectation" and its variants 61 times in the space of five pages.⁷ The title of this chapter is "Expectation as Determining Output and Employment." Chapter 12 is devoted to "The State of Long-term Expectation." From this one gains the impression that expectations weighed heavily on Keynes's mind, although Lachmann has warned us that Keynes was inclined to employ expectations when they suited his purpose and to disregard them when they did not.⁸ In the preamble to chapter 4, Keynes remarks that the role to be accorded to expectations was one of the things that perplexed him most in writing the *General Theory*.

An Austrian economist will thus not be too surprised to find a Post Keynesian ascribing an important role to expectations. Davidson, for example, argues that a key characteristic of the Post Keynesian approach is that "in a world where uncertainty is unavoidable, expectations have an unavoidable and significant effect on economic outcomes."⁹ In a similar vein, Robinson remarks that "if we cannot mention expectations, we cannot say anything at

all.”¹⁰ Anybody who has taken the time to study Shackle’s writings will have realized just how important a role Shackle accords to expectations.¹¹

An Austrian economist examining the literature will nevertheless also discover that there are Post Keynesians who do not ascribe a key role to expectations. In other words, there is disagreement in the Post Keynesian camp on what Davidson regards as one of its key characteristics. Garegnani, who is a member of the managing board of editors of the *Journal of Post Keynesian Economics*, argues that if expectations are granted a prominent role in economic analysis, it will not be possible to draw unambiguous conclusions. Accordingly, the correct procedure with regard to expectations is “to relate them uniquely to objective phenomena, so as to bypass them and relate the facts explaining the expectations directly to the actions of the individuals.” Expectations linked to such objective phenomena can then be regarded as “inescapable.”¹² John Eatwell and Murray Milgate have pointed out that this way of treating expectations has much in common with the rational-expectations approach, an approach that has come under criticism from Davidson.¹³ As the title of one of his articles suggests, it is “a fallacious foundation for studying crucial decision-making processes.”¹⁴

One wonders how the editor of the *Journal of Post Keynesian Economics* can claim that one of the key characteristics of the Post Keynesian approach is that it accords a vital role to expectations if a member of the managing board of editors of his journal disagrees with him on this score. The first lesson that Austrian economists have to learn from the Post Keynesians is that a Post Keynesian is not necessarily someone who attaches a great deal of importance to the role of expectations.

The way Garegnani and the rational-expectations approach treat expectations suggests that they are concerned with convergent expectations. Convergent expectations are at stake when different people entertain similar expectations of the future, and such convergence of opinion is likely to arise when there is some underlying structure on which such expectations can be based. Different people are likely to have similar views on when an eclipse is likely to occur, when the tides are likely to come in, and when the sun will rise. For Garegnani, the underlying economic structure is provided by a long-run center of gravitation dictated by the assumption of a uniform rate of profit. In the rational-expectations ap-

proach the underlying structure is normally a Walrasian model. If one wants to classify anticipations based on an underlying structure as expectations, it would help to classify them as convergent expectations, in which case they are hardly expectations at all, but rather part of existing knowledge. The appropriateness of this method of treating expectations depends on whether the underlying structure is flimsy or not.

There is a world of difference between such convergent expectations and the divergent expectations that arise when different people consider what the rate of interest or the price of copper might be 20 years from now.¹⁵ In the face of fundamental uncertainty, different people will entertain different expectations, and the divergence of views is symptomatic of the fact that there is no common structure on which different individuals might base their views. As Kregel has noted, when Lachmann talks about expectations, he has divergent expectations in mind.¹⁶ When Davidson refers to the importance of expectations in an uncertain world, he presumably also has divergent expectations in mind.

In drawing attention to the importance of divergent expectations in the kaleidic society, Lachmann lamented the fact that Austrians were slow to allot an important role to expectations, and that it was left to Shackle (and, we might add, Lachmann in 1943) to emphasize the importance of expectations.¹⁷ Those Austrians who dislike the indeterminacy introduced by expectations might therefore take comfort from the fact that there are Post Keynesians who agree with them that expectations are best avoided.

Even those Austrians who know very little about Keynes's approach may have come across his remark to the effect that each of us is dead in the long run. The second lesson that Austrians can learn from Post Keynesians is that not all Post Keynesians suffer from this aversion to long-run analysis. There are Post Keynesians who take the view that economic analysis should confine itself to the long-run relationships in which there exists a uniform rate of profit. Writers in this mold include Garegnani, Levine, Nell, Pasinetti, and Roncaglia. They are all members of the managing board of editors of the *Journal of Post Keynesian Economics*.

Not everybody in the Post Keynesian camp is convinced that the notion of a uniform rate of profit provides the economic system with a long-run center of gravitation. The essence of Harcourt's complaint is that the work of the equilibrating forces associated

with a uniform rate of profit will be interrupted by disequilibrating forces so that the long-run center of gravitation is unlikely to be reached.¹⁸ This critique is virtually identical to the one offered by Lachmann.¹⁹

Many Austrian economists know that the Post Keynesian notion of a long-run state of rest has come under fire from the Austrian camp. They will have to learn, however, that this notion has also come under fire from the Post Keynesian camp. Those Austrians who believe that a long-run center of gravitation will never be reached might have more in common with Davidson and Shackle than Davidson and Shackle have in common with those Post Keynesians who pin their hopes on the long run.

Keynes, Shackle, Lachmann, and Davidson maintain that we do not live in a deterministic world.²⁰ The presence of fundamental uncertainty makes it unlikely that an economy will gravitate to a predetermined state of rest. When there is fundamental uncertainty, divergent expectations will be the order of the day. These four authors maintain that mainstream economic theory takes an unduly deterministic view of the economic system. Davidson employs the term "ergodic" rather than "deterministic," but in the present context little is lost by regarding the two terms as interchangeable.²¹ In an ergodic world, the system will gravitate to a predetermined state of rest, wherever it starts from.

The third lesson that Austrian economists can learn from Post Keynesians is that there are Post Keynesians who take a deterministic view of economic processes. These are the Post Keynesians who pin their hopes on relationships existing under a uniform rate of profit. While the wheels of economic processes might grind slowly, they relentlessly (in this view) grind out a uniform rate of profit, whatever the starting point, which suggests that some Post Keynesians take an ergodic view of economic events.

Until now, Davidson's criticism of ergodicity has been confined to that which he finds amongst neoclassical economists (under whom he includes the Austrians). Although Austrian economists have much to learn from the Post Keynesians, they have yet to be informed whether the Ricardian notion of a uniform rate of profit represents an ergodic process or not.

When asked for his response on the question of determinism, Shackle remarked: "I think there is pretty complete indeterminacy."²² Lachmann attacked the notion of a deterministic economic

system whether he found it in the ranks of Ricardian,²³ Austrian,²⁴ or neoclassical economists.²⁵

One can argue that the Post Keynesian synthesis is an eclectic elephant with expectations for a trunk and a uniform rate of profit for a tail. The combination is likely to be a rather strange animal, designed by a committee.

There are thus at least three lessons that Austrians can learn from Post Keynesians. These lessons have to do with the role of expectations, the notion of a long-run state of rest, and the question of ergodicity. These three issues cannot be viewed independently of each other. Anybody who suggests that divergent expectations play an important role (Keynes, Shackle, Lachmann, Davidson) is unlikely to believe that the economic system is a deterministic one that converges on a long-run center of gravitation. Those who argue that economic forces will grind out a long-run state of rest appear to be taking an ergodic view of proceedings, and, if expectations play any role in such a view, they are likely to be of the convergent variety.

There is much common ground between the Post Keynesians and the Austrians. There are Austrians and Post Keynesians who feel that expectations should best be left out of economic analysis. There are Austrians and Post Keynesians who believe that economics is about the long run. There are Austrians and Post Keynesians who take a deterministic view of economic events.

If this is not enough, there is evidence of further common ground. There are, after all, Austrians and Post Keynesians who believe that divergent expectations play a major role in economics. There are Austrians and Post Keynesians who believe that economic theory should not be concerned with long-run situations. And there are Austrians and Post Keynesians who take a nondeterministic view of economic events.

NOTES

1. Jan Kregel, "Conceptions of Equilibrium: The Logic of Choice and the Logic of Production," in Israel Kirzner, ed., *Subjectivism, Intelligibility and Economic Understanding: Essays in Honor of Ludwig M. Lachmann on His Eightieth Birthday* (New York: New York University Press, 1986), 157-70 at 158.
2. Don Lavoie, "Euclideanism versus Hermeneutics: A Reinterpretation of Misesian Apriorism," in Israel Kirzner, ed., 192-210 at 192.

3. Geoff Harcourt, "Notes on an Economic Querist: G.L.S. Shackle," *Journal of Post Keynesian Economics* 4 (Fall 1981): 136-44 at 136.
4. "An Interview with Ludwig Lachmann," *Austrian Economics Newsletter* 1, no. 3 (Fall 1978): 15.
5. Ludwig Lachmann, "John Maynard Keynes: A View from an Austrian Window," *South African Journal of Economics* 51, no. 3 (September 1983): 368-379.
6. Ludwig Lachmann, "From Mises to Shackle: An Essay on Austrian Economics and the Kaleidic Society," *Journal of Economic Literature* 14 (1976): 54-62.
7. John Maynard Keynes, *The General Theory of Employment, Interest, and Money*, reprinted as vol. 7 of *The Collected Writings of John Maynard Keynes* (London: Macmillan, 1973).
8. Ludwig Lachmann, *The Market as an Economic Process* (Oxford: Basil Blackwell, 1986), 98.
9. Paul Davidson, "Post Keynesian Economics," in Daniel Bell and Irving Kristol, eds., *The Crisis in Economic Theory* (New York: Basic Books, 1981), 151-73 at 158.
10. Joan Robinson, *Further Contributions to Modern Economics* (Oxford: Basil Blackwell, 1980), 139-40.
11. George Shackle, *Epistemics and Economics: A Critique of Economic Doctrines* (Cambridge: Cambridge University Press, 1972).
12. Pierangelo Garegnani, "On a Change in the Notion of Equilibrium in Recent Work on Value and Distribution: A Comment on Samuelson," in M. Brown, K. Sato, and P. Zarembka, eds., *Essays in Modern Capital Theory* (Amsterdam: North Holland, 1976), 25-45 at 39; idem, "Notes on Consumption, Investment and Effective Demand: I," *Cambridge Journal of Economics* 2 (1978): 335-53 at 347; idem, "Notes on Consumption, Investment and Effective Demand: II," *Cambridge Journal of Economics* 3 (1979): 63-82 at 72; idem, "Notes on Consumption, Investment and Effective Demand: A Reply to Joan Robinson," *Cambridge Journal of Economics* 3 (1979): 181-87 at 183.
13. John Eatwell and Murray Milgate, "Unemployment and the Market Mechanism," in idem eds., *Keynes's Economics and the Theory of Value and Distribution* (London: Duckworth, 1983), 260-80.
14. Paul Davidson, "Rational Expectations: A Fallacious Foundation for Studying Crucial Decision-making Processes," *Journal of Post Keynesian Economics* 5, no. 2 (Winter 1982-1983): 182-98.
15. John Maynard Keynes, "The General Theory of Employment," *Quarterly Journal of Economics* 51 (1937): 209-23. Reprinted in *The General Theory and After, Part II: Defence and Development*, vol. 14 of *The Collected Writings of John Maynard Keynes* (London: Macmillan, 1973).
16. Jan Kregel, "On the Existence of Expectations in English Neoclassical Economics," *Journal of Economic Literature* 15 (1977): 495-500.

17. See reference in n6 above. See also Lachmann, "The Rôle of Expectations in Economics as a Social Science," *Economica* N.S. 10 (1943): 12-23.
18. Geoff Harcourt, "Marshall, Sraffa and Keynes: Incompatible Bedfellows?" in P. Kerr, ed., *The Social Science Imperialists* (London: Routledge and Kegan Paul, 1982), 205-21 at 218.
19. Ludwig Lachmann, *Macro-economic Thinking and the Market Economy* (London: Institute of Economic Affairs, 1973), 27.
20. See references in nn8, 11, 14, and 15 above.
21. Paul Davidson, "The Economics of Ignorance or Ignorance of Economics?" *CRITICAL REVIEW* 3, nos. 3-4 (Summer-Fall 1989): 467-887 at 477.
22. "An Interview with G.L.S. Shackle," *Austrian Economics Newsletter* 4, no. 1 (Spring 1983): 6.
23. See reference in n19 above.
24. Ludwig Lachmann, "On the Recent Controversy Concerning Equilibration," *Austrian Economics Newsletter* 2, no. 2 (Fall 1979).
25. Lachmann, *The Market as an Economic Process*, 158.

WHY THE ACRIMONY?
REPLY TO DAVIDSON

Our response to Davidson is two-pronged. First, we dispute the basis for his dismissal of Austrian economics as presented by O'Driscoll and Rizzo. In particular, we reject his claim, dictated entirely by his Post Keynesian perspective, concerning an "identical axiomatic foundation" of Austrian and neoclassical economics. Second, we seek to show that Davidson's criticism of neoclassicism (and by implication of Austrianism) is based on a superficial, incorrect, and outmoded reading of neoclassical economics.

Paul Davidson's recent assault on Austrian economics in general and on Gerald P. O'Driscoll, Jr., and Mario J. Rizzo's *The Economics of Time and Ignorance*¹ in particular ("The Economics of Ignorance or Ignorance of Economics?" CRITICAL REVIEW 3, nos. 3-4 [Summer-Fall 1989]: 467-87) raises a general issue peculiar neither to Austrian nor Post Keynesian economics—the ethics of book reviewing. As we will argue, Davidson is loath to muster the minimum of goodwill required to write a review article. While we readily concede that he touches on sore spots of both Austrian and neoclassical analysis, it is not so much the substantive issues he set forth (indeed, there is a lot of common ground here) that we find objectionable but his sloppy manner of reaching his conclusions.

Let us take our bearings. We do not assume here the task of

“defending” O’Driscoll and Rizzo’s particular version of Austrianism (or any other, for that matter) against Davidson’s onslaught. It is none of our business to speculate why the incriminated authors have elected to remain silent. Further, we take it as uncontroversial that these days there is no longer any need to justify a concern with modern Austrian economics. The samizdat years are definitely over. As we go along, we hope to expose Davidson’s curious procedure of playing off Austrian against neo-classical economics. In calling attention to serious gaps and flaws in Davidson’s portrayal of *both* Austrian *and* neoclassical analysis, there is no need for us to dwell on our view of the relationship between Austrian and neoclassical economics. For present purposes we are agnostic on this issue.

We are appalled at the vitriolic tone of Davidson’s outburst. Any hopes of a civilized dialogue to which one seemed to be entitled from the coauthor of *Economics for a Civilized Society* are shattered. Davidson is furious and he doesn’t hide it. What is it, then, that incurs Davidson’s wrath? Is it the blurb on the dust jacket boasting that “Austrian-subjectivist economics is the most original and powerful challenge to mainstream neo-classical economics,” whereas in fact it has of course been the Post Keynesians under Davidson’s inspired leadership who may justly claim the prize? Or is it that the sacred buzz words “time,” “money,” “uncertainty,” “expectations,” and “the importance of institutions” run the risk of being wrested from Davidson’s pontifical monopoly of definition? Or is it simply that the monster of Austrian economics, supposed to have been slain for good in the 1930s, now raises its ugly head again? If Austrian economics as presented by O’Driscoll and Rizzo is really built on such a feeble structure as Davidson makes it out to be, why does he brandish a sledgehammer to tear it down?

What is from our point of view perhaps most unsettling about Davidson’s review of O’Driscoll and Rizzo is that he is so reluctant to enter into a searching analysis of what the authors have to say, preferring instead to use the occasion to hammer away, for the umpteenth time, at what he would like to be identified as the distinguishing features of Post Keynesian economics. O’Driscoll and Rizzo,² who explicitly draw on Davidson’s summary of the main tenets of Post Keynesianism in mapping out their own territory, could hardly have foreseen that their extended hands would be so rudely slapped. To commit a *lèse-majesté* by expressing their

agreement with Davidson must have been the last thing on their minds.

Davidson's argument is straightforward enough: neoclassical economics is bad; Austrian economics is neoclassical economics in disguise; therefore, Austrian economics is just as bad. It may even be worse: Davidson laments that instead of using "scientifically" defined and established concepts such as "uncertainty" and "knowledge" in a way that would render feasible a comparison with what mainstream "economist-scientists" are saying, the Austrian economists invest them with "eccentric and often unexplained meanings, to achieve results that do not follow from the [shared] basic neo-classical premises."³

There are a couple of points here which strike us as deserving an immediate riposte. Davidson's harping on "scientific" concepts, "economist-scientists," "scientifically speaking," and so on not only makes for tedious reading. Donning the mantle of an economist-scientist (notice the hyphen) doesn't stand the leader of Post Keynesian economics in good stead: if there has ever been a philosopher-economist who, with good reason, was appalled at the notion of economics as a science it was Lord Keynes himself⁴—a sentiment shared, not for altogether different reasons, by F. A. Hayek.

It is also mildly amusing to find Davidson resorting to neoclassicism to ward off O'Driscoll and Rizzo's Austrianism. We wonder whether it has ever occurred to Davidson that he is accusing Austrian economists of adhering to the same strategy that he has (justly) castigated "mainstream" economists as guilty of pursuing when they impose ad hoc restrictions on their general equilibrium models in order to produce "Keynesian results."

Not surprisingly, fighting an Austrian straw man with a neoclassical straw man leaves Davidson stranded with his own Post Keynesianism. Q.E.D. Whether such shadow boxing is conducive to enhancing the quality of scholarly debate remains to be seen, though. In order to buttress our charge that Davidson in this case lacks the minimum goodwill essential for writing a critical rather than vicious review, we will strongly contest the terms Davidson brings to bear on his task. At one stage Davidson says, "I suspect that O&R have not understood Post Keynesianism."⁵ Similarly, we suspect that Davidson has not understood Austrianism as developed by O'Driscoll and Rizzo. By subjecting significant passages of

Davidson's essay to textual exegesis, we hope to expose the superficiality of Davidson's reading of both Austrian and neoclassical economics. And by focusing on what would have to be the case if a fruitful Austrian/Post Keynesian exchange were to occur, we shall draw attention to the acrimonious tone of Davidson's eruption.

Austrians and Post Keynesians

As Davidson would be the first to concede, the label "post-Keynesian" is an ambiguous term under whose umbrella a variety of theoretical approaches more or less peacefully cohabit, ranging from Shacklean economics to a range of projects seeking to marry classical political economy to the economics of Keynes⁶—with neoclassical (synthesis) Keynesians in between. Although in modern Austrian economics the variety is less pronounced, it is still there. For Davidson, however, Austrian economics presents itself just as monolithically as Post Keynesian economics. In fairness to Davidson, he should not be held responsible for not perceiving the different strands within Austrian economics: it was after all largely as a result of O'Driscoll and Rizzo's work that the different points of view had received an airing.⁷ It should also be recorded that the book had received a rather hostile reception in certain segments of the Austrian movement itself.⁸

But what is especially noteworthy here is that one of the most sympathetic and penetrating reviews was offered by an author whose Keynesian credentials are beyond any doubt. In a model of incisive composition, Thomas K. Rymes⁹ commended O'Driscoll and Rizzo's book for precisely the qualities Davidson wishes to dispute. Rymes notes the contrast between Arrow-Debreu neoclassicism and Austrianism, and points to the concerns shared by Austrians and fundamentalist Keynesians like Davidson. He regrets, though, that "the book ought to have had a long concluding chapter outlining the close correspondences and differences between the modern Austrians and the Keynesians, both of whom reject the concept of objective cost, Arrow-Debreu completable knowledge equilibria and the Paretian ranking of such equilibria."¹⁰

Davidson bemoans "an obvious lack of communication between Post Keynesians and Austrians," and he urges that "communication must precede any cross-fertilization."¹¹ We believe, on the contrary,

that the recent record of Austrian/Post Keynesian interchange, modest as it may be, is by no means as bleak as Davidson suggests.¹²

Perhaps the most perceptive contributions have come from two open-minded Post Keynesian scholars, Jan Kregel and Sheila Dow.¹³ In her *Macroeconomic Thought: A Methodological Approach*, Dow offers a comparative methodological appraisal of various schools of thought (Austrian, mainstream, Post Keynesian, Marxian) on such topics as microfoundations, the concept of equilibrium, the role of expectations and money, and macroeconomic policy, distinguishing between Cartesian/Euclidean and Babylonian modes of thought.¹⁴ The former relies on axioms, which are either true by definition or "self-evident," and involves using deductive logic to derive theorems. The ideal of closed systems of axiomatic logic finds its consummate expression in neo-Walrasian general equilibrium [henceforth GE] theory. The Babylonian (or Stoic/Roman) mode, on the other hand, calls into question the possibility of establishing a unique, incontestable set of basic axioms and warns that axiomatic errors may be compounded by the length of the chain of logic. Rather than relying on deductive reasoning from basic axioms, the Babylonian mode draws on several strands of arguments ideally reinforcing each other. While the unity of a Cartesian/Euclidean system of thought derives from the set of axioms from which all its theorems are deduced, "Babylonian thought is holistic in the sense that the binding factor of theories is a general perception of how the system as a whole works."¹⁵

We have referred to Dow's proposal not because we think her distinction between the Euclidean and Babylonian modes is without pitfalls¹⁶; nor do we agree with the details of her portrayal of Austrian economics. Nevertheless, we commend Dow's procedure because she grapples with the thorny issue of how we should go about making a comparative assessment of different schools of thought. Such an effort is missing in Davidson's article.

Davidson claims that Austrian and mainstream neoclassical economics share "the identical axiomatic foundation." While it is not at all clear what "the basic axioms of neoclassical economics" are, it is even less clear what the axioms of Austrian economics are. Ludwig von Mises's a priori true axiom of human action, which forms the starting point of his praxeological thinking, can safely be ruled out as sharing the axiomatic foundation with neoclassicism. Moreover,

as is well known, Mises's ideas on the philosophical foundations of economics have been a bone of contention within the Austrian school. But such considerations do not trouble Davidson. The Austrians, he says, subscribe to "all the underlying fundamental axioms of neoclassical analysis"¹⁷; and the axioms he confidently singles out are those of money neutrality, ergodicity, and gross substitution. According to Davidson, these are exactly the axioms whose removal was required by the Keynesian Revolution.

Enlisting Keynes, Davidson strongly contests¹⁸ O'Driscoll and Rizzo's claim that what separates schools of thought is not so much the different answers they give to the same questions, but the asking of different questions.¹⁹ Yet in comparing Post Keynesianism and Austrianism, O'Driscoll and Rizzo's suggestion may be much more to the point than Davidson concedes. Grossly simplified, the major difference between the two schools consists in what is deemed to be worthy of explanation. The overriding intellectual ambition animating Austrian economics is to present an explanation for the order-conferring, coordinative properties of market capitalism, while Post Keynesianism draws attention to the accident-proneness, endemic instability, and discoordination inherent in such a system.²⁰

It is ironic that Davidson²¹ should invoke Alan Coddington²² against O'Driscoll and Rizzo's view of uncertainty. Davidson is certainly on the beginning of a slippery slope here. Do we have to remind him that Coddington's diatribe was primarily directed against the Post Keynesians? In particular, Coddington²³ charged that the Post Keynesian interest in uncertainty is motivated by the desire to blame on market capitalism the chaos and waste ensuing from private production and investment decisions. But why, Coddington goes on to ask, should uncertainty as such go away when public-sector decisionmakers take over from private entrepreneurs? This seems an appropriate question to ask of Davidson's call, under "(nonergodic) economic conditions of true uncertainty," for governments to step in "to eliminate the anti-social results of decision making."²⁴ One wonders whether "nonergodicity" assumes the role here that "market failures" play in standard neoclassical economics.

To round off these considerations we will adduce two specific examples of Davidson's reckless reviewing style:

First, Davidson maintains that Austrians consider it "a fault" that

real agents cannot have complete knowledge of the future.²⁵ If Davidson had read O'Driscoll and Rizzo's chapter on knowledge, it would have occurred to him that far from constituting a limitation, agents' incomplete and incompletionable knowledge is accepted as part of the ontological furniture of Austrian economics. It cannot be overcome simply by an optimizing decision to expend resources on the acquisition of additional items of information. Therefore, it is inappropriate to criticize the Austrian view of the knowledge problem as if it were a variant of the economics of information, as Davidson does in arguing that "this costless mechanism . . . is delusive for it requires economic agents to possess a level of knowledge about the real world that they cannot *afford* to obtain."²⁶

Second, Davidson refers to O'Driscoll and Rizzo's discussion of the view of social institutions as efficient adaptations to the environment. Their unequivocal conclusion is: "in the absence of a clear conception of the nature of survival properties, we cannot know whether any given institution or course of action is the most adaptive. In fact, we know that many institutions will not be the most adaptive possible because of indivisibilities in the evolutionary process."²⁷ How, then, can Davidson possibly ask, "Does the claim of the Austrians' 'most powerful challenge' to neoclassicism ultimately rest on the flimsy tautological view that . . . those institutions and entrepreneurs that possess unspecified survival properties follow the Darwinian law of the survival of the fittest in the jungle of the marketplace?"²⁸

* * *

We reject Davidson's shrewd coupling of Austrian with neoclassical economics. In particular, we cannot accept his strong claims, dictated entirely by his own Post Keynesian perspective, concerning "the identical axiomatic foundation." This is of course not to deny, however, that there is an affinity between Austrian and neoclassical economics. There are important differences as well, but we have refrained from elaborating on them. It goes without saying that rejecting Davidson's approach to reviewing O'Driscoll and Rizzo does not thereby exonerate their work from all criticism.

We have eschewed entering into a debate on some of the important substantive issues Davidson mentions. But in order to have a lively and fruitful discussion, we first need to have an agreement on the rules of the game. Ultimately, we believe, the comparative

assessment will have to focus, in Dow's words, on the respective "general perception of how the system as a whole works," and not on the shared set of basic axioms.

TECHNICAL DISCUSSION: DAVIDSON ON NEOCLASSICISM

Additional examples of Davidson's penchant for making false allegations and then attributing them to the authors are given in the following technical discussion where we take a closer look at the formal issues raised by Davidson's attempt to link neoclassical general equilibrium (GE) theory with O'Driscoll and Rizzo's Austrian theory.

In support of his assertion that the authors' Austrian subjectivism is a particularly disgusting specimen of neoclassical theorizing, Davidson develops several loosely interconnected strands of argumentation: (1) With respect to the workings of market capitalism, O'Driscoll and Rizzo are said to rely on a socially Darwinistic version of neoclassicism, which enlightened neoclassical GE theorists like Frank Hahn would reject. (2) Where O'Driscoll and Rizzo claim originality for their "market-process" approach to time and uncertainty, Davidson is intent on maintaining that their theorizing can easily be subsumed within recent developments in neoclassical GE theory (e.g., sunspot equilibrium modeling). (3) Concerning their concept of market coordination in general and their concept of "pattern coordination" in particular, O'Driscoll and Rizzo are said to fall back ultimately on the GE notion of perfect coordination, with the implication that the former is not viable without an existence proof of the latter. Since the existence of a unique neoclassical general equilibrium depends, according to Davidson, on gross substitution—one of the "three restrictive axioms" unacceptable for Post Keynesians—the authors' notion of "pattern coordination" shares the fate of the GE concept.

1. *O'Driscoll and Rizzo and Enlightened Neoclassicism.* Davidson contends that both Austrian and neoclassical mainstream economics "ultimately develop a model of Social Darwinism via the marketplace."²⁹ Leaving aside the problem of the contextually correct meaning of the notion of Social Darwinism,³⁰ one wonders what the mechanistic Walrasian general equilibrium model of balancing market forces and the post-Paretian welfare theory of market failures have to do with nonmechanistic and promarket (Social) Darwinism. It is well known that leading GE theorists have either abstained from pro- or anti-market interpretations of their models,³¹ or else they have adopted a reserved, critical attitude toward promarket interpretations of the Arrow-Debreu theory of private ownership market economies.³²

Davidson goes on to say that "Arrow and Hahn (1971) . . . have proven that in an economy in real time and where contracts are made in terms of money, there need never exist any market solution which can coordinate all individuals' plans and clear all markets simultaneously."³³ Obviously, Davidson has in

mind here Chapter 14, "The Keynesian Model," comprising 23 of the 452 pages of Arrow and Hahn's canonical work.³⁴ Here Hahn presents an example (rather than a general model), which is "sufficient to show that if we take the Keynesian construction seriously, that is, as of a world with a past as well as a future and in which contracts are made in terms of money, no equilibrium may exist." But, "as usual, it is not possible to prove for the general case that no equilibrium exists."³⁵ In contrast to Hart's³⁶ nonexistence example in intertemporal general equilibrium theory, which initiated a voluminous literature on the existence of general equilibria with incomplete asset markets,³⁷ Hahn's nonexistence example of a short-period (temporary) general equilibrium has been completely neglected.

Considering both the insignificance of Hahn's example in Arrow and Hahn's *General Competitive Analysis* and its complete neglect—rightly or wrongly—in the subsequent literature on (intertemporal) general equilibrium theory since the early 1970s,³⁸ we cannot understand Davidson's insistence that "the Arrow-Hahn results must cast considerable doubts on the Austrian presumption that there always exists the possibility of a plan-coordinated equilibrium."³⁹ Even if other neoclassical nonexistence examples might destroy the logical consistency of O'Driscoll and Rizzo's project of an Austrian theory of markets, Hahn's example is unsuited to do so.

Next, Davidson refers to the negative (critical) role that neoclassical (better: neo-Walrasian) general equilibrium theory plays in discussions of problems "in the real world of time and uncertainty," based on its demonstration of "why an optimal allocation of resources and a complete coordination of all plans can *never* be achieved."⁴⁰ Despite the fact that most leading practitioners of GE theory do not justify their approach in this negative way,⁴¹ there is a logical problem with the Hahnian view of the negative role of GE theory: most conditions invoked to prove the existence and/or efficiency of general equilibrium states are not necessary but merely sufficient conditions—as noted by O'Driscoll and Rizzo.⁴² If any of the assumptions sufficient for the existence and/or efficiency of a general equilibrium do not hold, nothing at all can be said about, for instance, the validity of the claims of "practical men and ill-trained theorists" concerning "a beneficent and coherent rôle for the invisible hand."⁴³

2. *Sunspot Models and the Theory of Market Process.* Turning to more recent GE models incorporating "sunspot equilibria" à la Azariadis and Cass and Shell,⁴⁴ Davidson seeks to show⁴⁵ that O'Driscoll and Rizzo's argument to the effect that "existing 'price signals need not always be correct' . . . and hence that entrepreneurs may take short-run actions which create discoordination," is not original but is an implication of the neoclassical sunspot model. But the conclusion is beside the point when one considers the *ex ante* plan compatibility required for sunspot equilibria. It is one thing to maintain that "extrinsic randomness has real effects on prices and allocations,"⁴⁶ but it is quite another thing to say that "entrepreneurs may take short-run actions which create

discoordination." Davidson's invocation of sunspot models is correct only in so far as "extrinsic uncertainty cannot matter in the static Arrow-Debreu economy with complete markets," whereas "equilibrium allocations in which extrinsic uncertainty does play a role are Pareto optimal in a (*weaker*) sense which is appropriate to dynamic analysis."⁴⁷

We do not see how Cass and Shell's results encourage Davidson to contend that in a sunspot equilibrium "individuals misinterpret the cause of price changes, and therefore, take actions which adversely affect the short-run output and unemployment level of the economy."⁴⁸ There is no room for the notion of the short run in a sunspot equilibrium (i.e., a special class of intertemporal equilibria with rational expectations and finite planning horizon of agents).⁴⁹ Besides, what is the role of unemployment in intertemporal general equilibrium?⁵⁰ More specifically, one may also wonder what is the meaning of the long run in a sunspot equilibrium state when Davidson says, "Of course, in the long run, if entrepreneurs learn and therefore obtain information regarding the future, then they will not make continual 'systematic errors,' and full employment and plan-coordinated equilibrium will occur in both the sunspot models and the Austrian system." The point is neither that agents commit errors in the short run, nor that errors will somehow work themselves out in the long run, but rather that errors unrelated to sunspots ("systematic errors") will not occur, either in the short run or in the long run, simply because they are ruled out by the specification of the model.

In conclusion: Davidson's claim that there is nothing original with O'Driscoll and Rizzo's Austrian approach as compared with the sunspot equilibrium notion is not convincing because of his inadequate characterization of the latter.

3. *The Axiom of Gross Substitution and Pattern Coordination.* Under the heading of "The Three Restrictive Axioms of Neoclassical Analysis"⁵¹ Davidson asserts⁵² that O'Driscoll and Rizzo's weak equilibrium notion of "pattern coordination" presupposes the existence of a unique neoclassical general equilibrium price system based on the first of his "three restrictive axioms": the axiom of gross substitution, a condition "where every good (and service) is a good substitute for every other good (or service)." Since the so-called axiom of gross substitution is unacceptable to Davidson, and since without gross substitution a general equilibrium price system does not exist, O'Driscoll and Rizzo's notion of pattern coordination must, too, be rejected.

Although it may be argued that "pattern coordination" is a much broader concept than neoclassical general equilibrium (and therefore that Davidson's rejection of O'Driscoll and Rizzo's notion of pattern coordination is not warranted simply on account of this conceptual difference), our criticism is more fundamental: the way in which Davidson circumscribes the role of and the relationship between the notions of the existence and the uniqueness of general equilibrium price vectors, downward sloping excess demand curves, and

the so-called gross substitution axiom in neo-Walrasian GE theory is sloppy at best.⁵³

3.1 Existence and Uniqueness. In order to substantiate our claim, notice first that Davidson fails to differentiate between sufficient conditions for the existence and sufficient conditions for the uniqueness of general equilibrium price vectors.⁵⁴

It is well known that for the existence of a (not necessarily unique) general equilibrium price vector in a finite-dimensional world, continuity of excess demand functions (correspondences) on a nonvanishing vector field is sufficient, while in infinite-dimensional general equilibrium models certain boundedness conditions are additionally required to prove the existence of a general equilibrium price vector.⁵⁵ Thus, it seems clear that Davidson is dealing with sufficient conditions for the *uniqueness* of a general equilibrium price vector rather than conditions for its existence.

Concerning uniqueness one should further distinguish between conditions for the uniqueness of (1) nonstationary equilibria in finite-dimensional models and stationary (deterministic) equilibria in infinite-dimensional models; and (2) nonstationary intertemporal equilibria (deterministic or stochastic) in infinite-dimensional models. The notion of uniqueness and, thus, the downward slope and gross substitutability in excess demand functions are significant only with respect to the first type of equilibria. In the second type of equilibria nonuniqueness (indeterminacy) is generic but not singular. But even in the first type of models it is the nonuniqueness and the *multiplicity* of *locally* unique equilibrium solutions that has attracted, since the early 1980s, the attention of neo-Walrasian theorists—as testified by sunspot models and the “new” growth theory, for example.⁵⁶ Davidson is unperturbed by all this and is only concerned with a *globally* unique equilibrium solution in finite-dimensional models—a topic widely discussed in the 1950s and 1960s.

So Davidson is correct to stress the downward slope and gross substitutability in excess demand functions in the analysis of a unique equilibrium solution for finite-dimensional equilibria—*save sunspot equilibria*.

3.2 Downward slope and gross substitutability in excess demand functions. Students of neoclassical microeconomics may wonder about the significance—in a multi-commodity world—of the downward slope of the aggregate excess demand function; and why the negative reaction of the quantity demanded with respect to its own price should imply gross substitutability, which concerns the reaction of the quantity demanded with respect to changes in the price of other goods. In order to make sense of Davidson’s illegitimate extension to a multi-commodity setting of considerations only applicable to a two-commodity world, his two-commodity notion of the downward slope of the excess demand function should be replaced by Mas-Colell’s⁵⁷ concept of the monotonicity of the excess demand function. In addition, Davidson’s claim⁵⁸ concerning a simple relationship between the downward slope of excess demand

functions and gross substitutability should be supplemented by Mas-Colell's theorems on the relationship between the monotonicity of aggregate excess demand functions and the assumption of gross substitution.⁵⁹

3.3 Gross Substitution and Uniqueness. The significance of the axiom of gross substitutability in excess demand functions for a unique equilibrium in a (atemporal) pure exchange economy is illustrated by the following adaptation from Varian:⁶⁰ If every good is a gross substitute for every other good at the equilibrium price vector in a pure exchange economy, the equilibrium price vector is unique. Any two goods (i, j) are gross substitutes if and only if the excess demand of j increases if the price of i increases.

As demonstrated by Mas-Colell, not only Davidson is cognizant that the axiom of gross substitution is at best only indirectly applicable in production economies. Already since Rader's article⁶¹ GE theorists have been aware that in production economies this axiom is not directly applicable to all excess demand functions, because "the firms' demand functions for inputs cannot be expected to be gross substitutes."⁶² Moreover, as Kehoe⁶³ has recently shown for production economies, gross substitutability in consumers' demand functions does not *in general* imply a unique equilibrium solution. Put differently, in production economies the axiom of gross substitution is either inapplicable to all aggregate excess demand functions or, if applicable, it is not sufficient for a unique equilibrium solution.

To sum up: the axiom of gross substitution on which Davidson stakes his reputation is irrelevant in proving the *existence* of general equilibrium, and to the extent that uniqueness matters in contemporary GE modeling gross substitution is much less significant than Davidson seems to believe.

NOTES

1. Gerald P. O'Driscoll, Jr., and Mario J. Rizzo, *The Economics of Time and Ignorance* (New York: Basil Blackwell, 1985); Paul Davidson, "The Economics of Ignorance or Ignorance of Economics?" *CRITICAL REVIEW* 3, nos. 3-4 (Summer-Fall 1989): 467-87.
2. O'Driscoll and Rizzo, 9.
3. Davidson, 477.
4. On Keynes's conception of economics, see, e.g., Rod M. O'Donnell, *Keynes: Philosophy, Economics and Politics* (London: Macmillan, 1989), ch. 8.
5. Davidson, 472.
6. For a useful survey, see, e.g., Omar Hamouda and Geoffrey C. Harcourt, "Post-Keynesianism: From Criticism to Coherence," in John Pheby, ed., *New Directions in Post-Keynesian Economics* (Brookfield, Vt.: Edward Elgar, 1989).
7. Cf. the respective reviews of *The Economics of Time and Ignorance* by Israel Kirzner and Ludwig Lachmann in *Market Process* 3, no. 2 (Fall 1985).

8. Charles W. Baird, "The Economics of Time and Ignorance: A Review," *The Review of Austrian Economics*, 1 (1989): 189–206.
9. Thomas K. Rymes, "Review of *The Economics of Time and Ignorance*," *Canadian Journal of Economics* 20 (February 1987): 190–92. See also the favorable review by the late Kenneth E. Boulding, *American Journal of Sociology* 92, no. 4 (January 1987).
10. Rymes, 192.
11. Davidson, 472.
12. See the literature cited in Stephan Boehm, "Subjectivism and Post-Keynesianism: Towards a Better Understanding," in Pheby, ed., *New Directions* (n6 above). See also Bruce J. Caldwell, "Post-Keynesian Methodology: An Assessment," *Review of Political Economy* 1, no. 1 (1989): 43–64; and the response by Sheila C. Dow, "Post-Keynesian Methodology: A Comment," *Review of Political Economy* 4, no. 1 (1992): 11–13; and various contributions in K. Groenfeld, J.A.H. Maks, and J. Muysken, eds., *Economic Policy and the Market Process: Austrian and Mainstream Economics* (Amsterdam: North Holland, 1990).
13. Jan A. Kregel, "Conceptions of Equilibrium: The Logic of Choice and the Logic of Production," in Israel M. Kirzner, ed., *Subjectivism, Intelligibility and Economic Understanding: Essays in Honor of Ludwig M. Lachmann on his Eightieth Birthday* (New York: New York University Press, 1986); Sheila C. Dow, *Macroeconomic Thought: A Methodological Approach* (Oxford: Basil Blackwell, 1985).
14. Dow, *Macroeconomic Thought*, 11–17.
15. *Ibid.*, 16 (emph. added).
16. See Bruce J. Caldwell, "Review of Sheila Dow, *Macroeconomic Thought: A Methodological Approach*," *Economics and Philosophy* 2, no. 1 (April 1986): 141–44.
17. Davidson, 474 (emph. in the original).
18. *Ibid.*, 473.
19. O'Driscoll and Rizzo, 17, 229–30. Davidson conveniently hides that it was Joan Robinson who had come up with this suggestion.
20. From an Austrian perspective, see Israel M. Kirzner, *The Meaning of Market Process* (London: Routledge, 1992), ch. 1, which is, *inter alia*, a response to Jan Kregel's contribution cited in n13 above.
21. Davidson, 476.
22. Alan Coddington, "Deficient Foresight: A Troublesome Theme in Keynesian Economics," *American Economic Review* 72, no. 3 (June 1982): 480–87.
23. *Ibid.*, 486.
24. Paul Davidson, "Is Probability Theory Relevant for Uncertainty? A Post Keynesian Perspective," *Journal of Economic Perspectives* 5, no. 1 (Winter 1991): 129–43 at 142.
25. Davidson, "The Economics of Ignorance," 469.
26. *Ibid.*, 468 (emph. added). Cf. Davidson's equally misleading n1, at 483.

27. O'Driscoll and Rizzo, 40.
28. Davidson, "The Economics of Ignorance," 470.
29. *Ibid.*, 467.
30. Davidson may be interested to hear that the notorious Austrian Darwinist social philosopher Hayek is a fierce critic of "Social Darwinism"—the idea that cultural evolution be studied along the lines of biological evolution—and considers it to be a "tragic mistake." See, e.g., F. A. Hayek, *The Fatal Conceit: The Errors of Socialism* (London: Routledge, 1988), 23-28.
31. Gerard Debreu, *Theory of Value: An Axiomatic Analysis of Economic Equilibrium* (New York: John Wiley, 1959).
32. See, e.g., Kenneth J. Arrow, "A Cautious Case for Socialism," in Irving Howe, ed., *Beyond the Welfare State* (New York: Schocken Books, 1982); Frank H. Hahn, *On the Notion of Equilibrium in Economics* (Cambridge: Cambridge University Press, 1973); *idem*, "Keynesian Economics and General Equilibrium Theory: Reflections on Some Current Debates," in Geoffrey C. Harcourt, ed., *The Microeconomic Foundations of Macroeconomics* (London: Macmillan, 1977); *idem*, "General Equilibrium Theory," in Daniel Bell and Irving Kristol, eds., *The Crisis in Economic Theory* (New York: Basic Books, 1981); and *idem*, "Economic Theory and Keynes' Insights," *Empirica* (Austrian Economic Papers) 11, no. 1 (1984): 5-22.
33. Davidson, "The Economics of Ignorance," 468.
34. Kenneth J. Arrow and Frank H. Hahn, *General Competitive Analysis* (San Francisco: Holden-Day, 1971).
35. *Ibid.*, 361. Referring to the same passage, Davidson cavalierly comments: "As Arrow and Hahn . . . demonstrated, *all* general equilibrium existence proofs are jeopardised in a world with fixed money contracts over time" ("A Technical Definition of Uncertainty and the Long-run Non-Neutrality of Money," *Cambridge Journal of Economics* 12 [1988]: 329-37 at 336; *emph. added*).
36. Oliver D. Hart, "On the Optimality of Equilibrium when the Market Structure Is Incomplete," *Journal of Economic Theory* 11 (1975): 418-43.
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41. See, e.g., Roy Radner, "Intertemporal General Equilibrium," in Lionel W. McKenzie and Stefano Zamagni, eds., *Value and Capital Fifty Years Later* (London: Macmillan, 1991).
42. O'Driscoll and Rizzo, 82.
43. Hahn, *On the Notion of Equilibrium in Economics*, 14-15.
44. Costas Azariadis, "Self-fulfilling Prophecies," *Journal of Economic Theory* 25

- (1981): 380–96; David Cass and Karl Shell, “Do Sunspots Matter?” *Journal of Political Economy* 91 (1983): 193–227.
45. Davidson, “The Economics of Ignorance,” 470.
46. Stephen E. Spear, “Sufficient Conditions for the Existence of Sunspot Equilibria,” *Journal of Economic Theory* 34 (1984): 360–70 at 360.
47. Cass and Shell, 193 (emph. added).
48. Davidson, “The Economics of Ignorance,” 470.
49. To emphasize, Cass and Shell present a finite-dimensional model. In parentheses, it should be noted that in infinite-dimensional models one may counterpose stationary and nonstationary intertemporal equilibria. Accordingly, the first type would refer to the long run and the second to the short run. But if so, Davidson would have to make up his mind.
50. In intertemporal equilibrium unemployment is excluded by definition.
51. It should be noted that what Davidson calls “axioms” are “assumptions” for mathematical economists.
52. Davidson, “The Economics of Ignorance,” 474.
53. Ibid.
54. The same point is made by Hahn, *On the Notion of Equilibrium in Economics*, 8, in criticism of Kaldor.
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58. Davidson, “The Economics of Ignorance,” 474.
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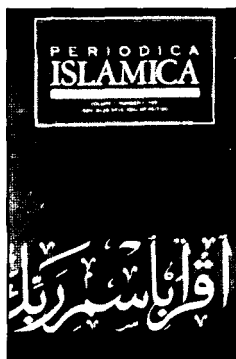
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AUSTRIANS AND POST
KEYNESIANS ON ECONOMIC
REALITY: REJOINDER TO CRITICS

Most economists—old and new classical, old and new Keynesian, and Austrian (as embodied in O'Driscoll and Rizzo's The Economics of Time and Ignorance) postulate an immutable reality unchangeable by any human action (the ergodic hypothesis). They differ only over the amount of information decisionmakers have, in the short run, about this unchanging reality. Keynes and the Post Keynesians provide an axiomatic alternative model that presumes a transmutable economic reality (i.e., it postulates a nonergodic environment). Runde, Torr, Prychitko, and Boehm and Farmer fail to adequately address this dichotomous analysis of reality in responding to my review of O'Driscoll and Rizzo's book.

"In economics you cannot *convict* your opponent of error; you can only *convince* him of it. And, even if you are right, you cannot convince him if there is a defect in your own powers of persuasion and exposition or if his head is already so filled with contrary notions that he cannot catch the clues to your thought which you are trying to throw to him."¹

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Paul Davidson, the Holly Chair of Excellence in Political Economy, Department of Economics, University of Tennessee, Knoxville, TN 37996, telephone (615) 974–4221, telefax (615) 974–1686, is editor of the *Journal of Post Keynesian Economics* and author, *inter alia*, of *Post Keynesian Macroeconomic Theory* (Edward Elgar, 1994), *Can The Free Market Pick Winners?* (M. E. Sharpe, 1993), *Controversies in Post Keynesian Economics* (Edward Elgar, 1991), and, with Greg Davidson, *Economics for a Civilized Society* (W. W. Norton, 1988).

So it appears to be with Runde, Torr, Prychitko, and Boehm and Farmer. To different degrees they have not grasped what is at issue between my thought and that of O'Driscoll and Rizzo² (hereafter O-R). The fault may partly lie in my choice of lexicon. I was trying to show that there is a paradigm's worth of difference between O-R's representation of Austrian economics and what I call Post Keynesian analysis. This demonstration presumed readers with enough training in economics to be familiar with the mainstream economists' claim that their approach to economics is the *only* "scientific" methodology for the study of economics. Consequently, I adopted the dialect of mainstream theorists as a basis for establishing "scientific" differences amongst theories. Had my exposition been less jargon based, Austrians such as Prychitko, Runde, and Torr, who prefer Austrian and/or philosophical lexicons, might have caught more of the clues I was throwing out.

Boehm and Farmer (hereafter B-F), on the other hand, complain about what they describe as my "superficial" simplified discussion of Walrasian general equilibrium (hereafter GE) theory. B-F would have preferred no simplifications; rather they want a precise jargon-laden retelling and analysis of recent variants of the basic GE theory, even though the primary focus of my review was not on GE models but on the uncertainty aspects of Austrian and Post Keynesian systems.

Accordingly, before responding directly to the criticisms of these scholars, let me attempt to clarify any defects in my earlier exposition that may have induced some of their criticisms.

Is Reality Immutable or Transmutable?

When discussing the implications of *uncertainty* on economic activity, the fundamental difference between various schools of economic thought involves each school's conception of the external economic reality in which decisionmakers operate. Eighteenth- and nineteenth-century classical economists assumed that economic agents, operating in a world of perfect certainty, had *full knowledge* of an *immutable external economic reality* that governed all past, present, and future economic outcomes. While rejecting perfect certainty models, most economists today still accept as a universal truth the existence of an immutable reality. (This unquestioned

belief is what I called the ergodic axiom.) Today's mainstream economists typically believe that this immutable reality can be described by objective probabilities.

The main distinction between the perfect certainty models of an earlier day and today's orthodox models is that the latter presume that for decisionmakers to reliably forecast future outcomes they must calculate probabilities³ by processing "information" embedded in past and present market data. Future outcomes are conceived as merely the statistical shadow of the past. In the short run, if individual decisionmakers have not estimated (or are unable to estimate) these objective probabilities correctly, they possess erroneous views about the risky (uncertain) future. In the long run, those who do calculate the objective probabilities correctly will survive. Those who fail to make correct estimates will die. In an immutable world, this Darwinian process weeds out the latter group, which makes persistent systematic errors.

B-F, on the other hand, indicate that "agents' incomplete and incompletable [*sic*] knowledge is . . . part of the ontological furniture of Austrian economics" (413). Of course, rational-expectations theorists also do not claim that the agents in their models obtain *complete* knowledge of reality. Rational expectations only require agents to obtain reliable probabilistic knowledge from their processing of market signals. Is the main difference between the ontological furniture of rational-expectations theorists and Austrians only the degree of statistical reliability (i.e., the variance) of the knowledge agents can possess?

B-F argue that in Austrian theory agents' lack of complete knowledge "cannot be overcome simply by an optimizing decision to expend resources on the acquisition of additional items of information" (413). Do B-F mean that the cost of processing additional information to obtain more (not necessarily complete) reliable knowledge is infinite?⁴ O-R argue that "there is no reason to believe entrepreneurs will systematically be in error,"⁵ even if the costs of calculating future probabilities are infinitely high. O-R must be presuming that in the long run entrepreneurs' decisions are compatible with an external immutable reality.⁶

Beginning with Keynes, however, a few economists have developed an alternative conception of an external reality that, at least in some economic dimensions, is *transmutable* or *creative* in that its future can be created by human actions. For some economic vari-

ables, even if conditional probabilities govern past and/or current events, this information is not relevant for forming correct expectations about the future. In other words, even if agents have the capacity to process market information regarding past and current outcomes, this data will not help them reliably forecast the future. As Keynes wrote, "About these [future] matters there is no scientific basis to form any calculable probability whatever. We simply do not know."⁷

In sum, as Table 1 indicates, there are only two alternative axiomatic conceptions of reality. Reality is either (A) *immutable* or it is (B) *transmutable*.⁸

Immutable Reality

If reality is immutable, then society cannot enact laws (policies) to alter inevitable probabilistic future outcomes any more than a legislature can overturn the "law of gravity" or the probability distribution associated with the game of roulette. In this conception, humans have no freedom to alter their long-run economic future.⁹ The State cannot have any more "information" about the future than individuals, and may have less. This view results in the Ronald Reagan-type rhetorical question, "How can bureaucrats in Washington know better how to spend your money than you do?"

The only issue for immutable reality theorists is whether agents obtain reliable information regarding the future from market signals. If human beings can obtain this information, what does it cost to process it? If people cannot obtain or process this information, does some inanimate thing — a Turing machine — exist that can process the information for society?

Table 1 indicates that there are two types of immutable reality models. Type 1 is distinguished from type 2 by different assumptions regarding how much reliable information about the immutable reality can be obtained by agents in the short run. Both types share the identical axiomatic foundation regarding the existence of an immutable reality.

Type 1 immutable-reality models presume that at the initial instant agents already reliably "know" the future. Type 1 models include old classical perfect certainty models, those New Classical and New Keynesian models that assume, in the short run, that

Table 1: Concepts of External Economic Reality

A. IMMUTABLE REALITY (AN ERGODIC SYSTEM)

Type 1. In the short run, the future is known or at least knowable.

Examples of theories using this postulate are:

- (a) Classical perfect certainty models
- (b) Actuarial certainty equivalents, such as rational expectations models
- (c) New Classical models
- (d) Some New Keynesian theories.

Type 2. In the short run, the future is not completely known due to some limitation in human information processing and computing power.

Examples of theories using this postulate are:

- (a) Simon's bounded rationality
- (b) Savage's expected utility theory
- (c) New Keynesian models such as asymmetric information and coordination failure theories
- (d) Austrian theory.

B. TRANSMUTABLE OR CREATIVE REALITY (A NONERGODIC SYSTEM)

Some aspects of the economic future are created by human action today and/or in the future. Examples of theories using this postulate are:

- (a) Keynes's *General Theory*
- (b) Post Keynesian monetary theory
- (c) Post-1974 writings of Sir John Hicks
- (d) G.L.S. Shackle's crucial experiment analysis.

agents have rational expectations, and all other models in which agents know actuarial certainty equivalents.

Type 2 immutable-reality models assume that, in the short run, agents' knowledge regarding reality is "incomplete" or unknown. Ignorance about some aspect(s) of the immutable economic reality is the hallmark of such models. Type 2 models encompass all systems that presume some limitation on human processing and/or computing power that prevents agents from using market data to obtain reliable estimates of objective probabilities. Expected utility theory, Simon's bounded rationality, Austrian economics, and asymmetric information models¹⁰ are examples of type 2 models. In the long run, however, in both type 1 and type 2 models, objective reality is somehow revealed to all successful market participants, or, at least, they behave "as if" they knew reality.

Except for the perfect certainty case, immutable-reality models

typically employ a subjectivist orientation according to which agents form expectations (usually, but not necessarily, in the form of Bayesian subjective probabilities). In the short run, subjective probability is treated as a type of knowledge that need not match the immutable external reality that is presumed to exist. Today's decisionmakers can make short-run errors if they do not possess sufficient mental processing power (even if past and present market data exist) to reliably "know" the objective probabilities regarding the future. In the long run, however, as agents "learn," subjective probabilities will tend to converge with the objective probabilities that represent the unchanging external reality. O-R take it as an article of faith that "learning will take place."¹¹ Those agents whose subjective probabilities do not converge to the objective probabilities will make persistent systematic errors involving losses in income. These persistent error makers do not learn and therefore cannot survive in the Darwinian free marketplace.¹² In the long run, the surviving rational agents make optimal choices as their subjective expectations adapt to the immutable reality.¹³

In sum, old and New Classical theories, O-R's Austrian economics, as well as old (neoclassical synthesis) Keynesian and new Keynesian theory, share the fundamental belief in an immutable external economic reality. This shared belief in an immutable external economic reality is what I have labeled the ergodic axiom of classical economics.¹⁴ Samuelson has made the acceptance of the ergodic hypothesis the sine qua non of the scientific method in economics.¹⁵ According to Samuelson's criterion, therefore, all transmutable reality models, including Keynes's *General Theory*, are unscientific.

Are Probabilities Knowable but Unknown Because of Limited Human Reasoning Power?

As Table 1 indicates, mainstream economists fall into two camps. The most orthodox and logically consistent classical brethren presume individuals are capable of interpreting past and present market signals in terms of reliable estimates of future behavior, even in the short run. The less orthodox classical camp, which includes Austrian economists, believes the future is "uncertain" because to-

day's human beings are simply not capable of reliably estimating the objective probabilities that describe reality.

Frank Knight, for example, explicitly distinguished between quantifiable risk and uncertainty, writing, "The practical difference between the two categories, risk and uncertainty, is that in the former the distribution of the outcome in a group of instances is known (either through calculation *a priori* or from the statistics of past experience), while in the case of uncertainty, this is not true."¹⁶ Uncertainty occurs because we possess only "partial knowledge" of the cosmos.¹⁷ Knight argued that "objective phenomenon [reality] . . . is certainly knowable to a degree so far beyond our actual powers . . . [and therefore] any limitation of knowledge due to lack of real consistency in the cosmos may be ignored."¹⁸ In other words, Knight believed in the uniformity and timeless consistency (immutability) of nature.

For Austrians the Market Is a Turing Machine

As I noted in my review of O-R,¹⁹ their brand of Austrian economics posits an immutable external reality that works in the same way nineteenth-century physicists thought the physical world works. In their emphasis on uncertainty, however, Austrians differ from mainstream old and New Classical theorists. Austrians believe that external reality is too complicated for any single human being ever to process the information about it being sent out by market signals. As B-F indicate, for each individual, knowledge about the future is "incompletable" [*sic*].

It is the free market that mediates the myriad random decisions and produces an evolutionary process where only the fittest survive by making what are (in hindsight) the proper decisions. The free market is the Turing machine Austrians rely on for coordinating plans and outcomes in a world of uncertainty, a world where no individual can calculate the objective probabilistic risks.

Alan Turing, an English mathematician, is best known for cracking the Germans' "Enigma" code during the Second World War. As a mathematician, however, Turing is famous for discovering the Turing machine, a hypothetical device for performing any mathematical calculation that is amenable to the application of rules. Turing demonstrated that if Nature obeys immutable rules, then a

Turing machine can, in the long run, always provide a calculable future. Thus, if one assumes that economic processes always conform to immutable mathematical laws or rules, whether human intuition knows the laws or not, then the future is conceptually predictable.

Keynes, Uncertainty, and a Transmutable (Creative) Reality

For Keynes and the Post Keynesians, on the other hand, uncertainty means a transmutable reality. Uncertainty entails situations in which decisionmakers believe that no relevant information exists today that can be used as a basis for scientifically predicting future events. As John Hicks has put it, "One must assume that people in one's models do not know what is going to happen, and know that they do not know what is going to happen. As in history!"²⁰

Consequently, probabilistic risks, conceptually knowable on the basis of past and present observations, must be distinguished from uncertainty. Probabilistic risk (ergodicity) may characterize routine, repeatable economic decisions where it is reasonable to presume an immutable reality. As Keynes showed in *The General Theory*, however, for important economic decisions involving investment, the accumulation of wealth, and finance, agents are dealing with an uncertain, nonprobabilistic *creative economic external reality* in which today's human actions can create a new and different future reality.²¹

This Keynes-Post Keynesian conception of transmutable reality is logically incompatible with the ergodic postulate of classical axiomatic value theory upon which all classical theories as well as mainstream Keynesianism are based. Keynes's analytical system, therefore, is truly a revolutionary logical way of analyzing a money-using, entrepreneurial market system.

Transmutable reality implies the possibility that there is a permanent, positive role for government in designing policies and institutions to provide results preferable to those that might be generated by competitive markets in the real world. Such policies and institutions can create a new future reality for the economy in which we live. Through institutional and policy changes, society can intelligently control and improve the performance of the economy compared with what would occur under *laissez-faire*, com-

petitive market conditions. Keynes's suggestion that government ensure that there never be a lack of effective demand²² produces a Keynesian (production) efficiency (a gain in production equal to the gain in total output at full employment over the equilibrium output in a free market system) that significantly outweighs any allocative inefficiency that might be induced.²³

In contrast, when an immutable reality is postulated, it logically follows that, in the long run, all policy interventions that limit market adjustments are likely only to make economic matters worse by reducing the allocative efficiency implied in a laissez-faire system. Keynesian effective demand policies cannot produce output in excess of what is predetermined by this reality.

Response to Runde

Runde misinterprets me when he claims that I have substantially different official and unofficial ontological and epistemological views (382). I have consistently argued that information about the future does not currently exist and that agents know that they don't know the future. The market is therefore not a Turing machine, even in the long run!

Runde incorrectly believes that I accept Samuelson's criterion of science as the "identification of empirical regularities." Since I reject the ergodic axiom, I cannot accept Samuelson's definition of science.²⁴ One of my objectives was to explain why Samuelson's ergodic axiom, "what Bhaskar calls empirical realism" (384), is neither a necessary nor a sufficient condition for "science." The primary goal of science is to explain. If one can also predict on the basis of "empirical regularities," then that is icing on the scientific cake. Scientists, however, should "know" in what areas they cannot search for past empirical regularities to predict the future. In these nonergodic areas of economic science, human beings can and should develop "certain important [institutional] factors which somewhat mitigate in practice the effect of our ignorance of the future."²⁵ The development of additional institutional factors to correct flaws as they develop in the existing market system is a goal of Post Keynesian economic analysis.

Things cannot be "more or less uncertain than others," as Runde claims (385). Some future events are uncertain (nonergodic); the rest

are risky (ergodic) and therefore are, at least conceptually, actuarially certain. There is, in my view, no room for "probable knowledge," a category that Runde attempts (385-86) to pin on me. Of course, in the short run, agents can rightly or wrongly believe that A is ergodic, B is nonergodic, or even that C is ergodic sometimes and nonergodic at other times. Only the future will tell whether any or all of these beliefs are correct.

Runde claims most "Austrian economists would agree that economic circumstances are *invariably* 'generated in nonergodic circumstances'" (385, emphasis added). If they do, then they are being logically inconsistent. Prychitko (373-74) recognizes this when he notes that "most Austrians have traditionally argued . . . that . . . unhampered market processes tend toward general equilibrium . . . [but they have not shown] how market processes have a fundamental convergence property without relying upon assumptions that may threaten their allegiance to subjectivism and uncertainty." Logically consistent Austrians, such as Knight and Hayek, did not believe in a transmutable reality. In the lexicon of Knight, a transmutable cosmos is an oxymoron.

Sensible economic expectations are not "mere stabs in the dark," as Runde suggests (385), as long as human beings "know" that the state will enforce (and most people will obey) the civil law of contracts. By entering into long-term cash-inflow and cash-outflow contracts, people can have sensible expectations regarding their future net cash-flow position. That is what makes the Keynesian concept of liquidity the keystone in understanding why money-using entrepreneurial economies can become stuck at an unemployment equilibrium between, say 2 percent and 25 percent unemployment. High unemployment rates, by causing a massive number of bankruptcies that void myriad cash-flow contracts without recourse, can threaten the entire financial structure of these economies. This liquidity crisis may be the answer to Prychitko's query (371-72) as to why market systems do not generate 90 percent unemployment equilibria. It is a question of liquidity, a question that seems to be far from a central concern of Austrian economics.

Citing work that Keynes virtually completed in the early 1900s and published in 1921, Runde claims that Keynes would "recommend the assignment of equal probabilities" (395n17) to explain how an agent would make a decision in the absence of any information. Yet, the older and wiser Keynes of *The General Theory* argued that

we should not “rationalise our behaviour by arguing that to a man in a state of ignorance errors in either direction are equiprobable . . . for it can be shown that the assumption of arithmetic equal probabilities based on a state of ignorance [uncertainty] leads to absurdities.”²⁶ I accept Keynes’s more recent message as consistent with his revolutionary *General Theory*. Runde apparently does not.

An important Post Keynesian theme is almost a throwaway line in Runde (392). Post Keynesians believe that the more stable and enduring features of our social world are not empirical regularities based on “natural” structural relationships. Economic variables, to the extent that they are sticky over time, usually owe this regularity to created social structures. Since human social structures can and do change due to human action, economic reality is transmutable.

Runde (388) is incorrect when he assumes that the institution of forward money contracts exists independently of my nonergodic analysis. Although the term “forward contracts” is prominent in Arrow-Debreu GE theory, the GE forward contract is conceptually quite different (because of its ergodic axiomatic foundation) than the forward contracts of either Post Keynesian theory or the real world. The forward contract of the world in which we live, as well as of the Keynes-Post Keynesian analysis, is an agreement signed on the spot specifying delivery *and* payment at a specific future date. Roy Radner indicates that in GE theory a forward contract specifies a forward delivery date but requires *instantaneous payment*.²⁷ Hence in GE theory there can never be a liquidity problem in terms of meeting one’s future nominal contractual liabilities. Liquidity questions are prominent only when the institution of forward contracts specifies a specific future date for nominal payments in a transmutable reality setting.

Response to Torr

Torr’s criticism is twofold. First, he indicates that Austrians are not monolithic in their analysis. Some, such as Shackle and Lachmann, believe in a transmutable reality. Lachmann thought Austrians “had something to learn from Keynes, Shackle and Davidson” (Torr, 400). Second, Torr suggests that Post Keynesians are not monolithic either. Torr notes that several members of the board of editors

of the *Journal of Post Keynesian Economics* use an immutable reality framework. Since there is an apparent diversity of views among Austrians and Post Keynesians on the ergodic axiom, Torr takes me to task for (1) claiming a key role for expectations in a nonergodic environment when "a member of the managing board of editors . . . disagrees with" me (401) and (2) not recognizing a similar Austrian diversity of analysis. Prychitko and B-F (in slightly different contexts) raise the same diversity issue.

First, my failure to mention the apparent diversity among Austrians is easily explained. I was asked to review O-R's book. I was not asked to compare and contrast Austrian economics as it appears in all its diverse forms with all the different ideas of members of the board of editors of the *JPKE*. Whether some Austrian theorists are really closet Post Keynesians or even true-blue hybrids, I leave for others to discuss.²⁸

To my mind, George Shackle remained until the end a very humane Austrian who did not believe that the market could be trusted to be a Turing machine. Accordingly, despite his friendship with and admiration of Hayek (Torr, 400), Shackle's logical analysis is not consistent with Hayek's vision of equilibrium. In fact Shackle's analysis led him to the other extreme, namely, the belief that the economic system is inherently unstable. Shackle downplayed the possible link between his view and the emphasis of Keynes and the Post Keynesians on the importance of social structures (e.g., laws and traditions) to mitigate the influences of uncertainty.

Second, that members of the board of editors of the *JPKE* have developed various models that are sometimes incompatible with what I have identified as the essential axiomatic foundation of Post Keynesian theory is obviously true. Why this diversity?

In 1982, in the first chapter of my *International Money and the Real World*,²⁹ I described the then-developing Post Keynesian literature as an "amalgam" that did not represent a monolithic or puristic logical approach. Davidson, Weintraub, Minsky, and even Shackle were among those that I classified as members of the Keynes school. Others that I identified in the Post Keynesian amalgam included Radicals (e.g., Galbraith), neo-Keynesians (who now prefer to be called neo-Ricardians or Sraffians)³⁰ and even some old Keynesians (e.g., the post-1974 Hicks).³¹ The main glue that bound this mixture together was a dislike of neoclassical economics and a desire to develop a superior alternative to it. Thus, at this early

stage of developing a Post Keynesian literature, a basic criterion for joining the Post Keynesian club was “the enemy of my enemy is my friend.” This maxim, as Torr rightly recognizes, often makes strange bedfellows.

As founding editors of the *JPKE* in 1978, Sidney Weintraub and I tried to cast our net as wide as possible within this amalgam in establishing an editorial board. The purpose was to promote a wide-open discussion in these early formative years. A cynic might suggest that all non-sinners and even repentant sinners could join the club; Post Keynesians were not exclusive. Torr merely has correctly noticed that the neo-Ricardian contingent of the earlier Post Keynesian amalgam has never cast off its classical ties. These economists’ fear of mathematical indeterminacy and the inability to predict with the precision of Samuelson-type scientists caused them to bury the Keynes garb they once tried to wear.

Those Austrians who felt queasy about the kaleidic approach of Shackle and Lachmann, like the neo-Ricardians, apparently prefer the comfort of presuming an immutable reality where, as O-R state, “there is no reason to believe entrepreneurs will be *systematically* in error.”³² O-R’s “vague Darwinian process” presupposes an unchanging external reality in which Nature is always “weeding out” those with “inferior survival properties,”³³ that is, those with propensities to make persistent systematic errors.

Response to Prychitko

Prychitko’s comments are basically sympathetic to my position. I found that O-R, whom I took as representing modern Austrian thought, had not demonstrated logically that in a world where humans were truly ignorant of the future, that, even in the long run, a laissez-faire market system would have a propensity to establish a full employment equilibrium where agents’ plans were coordinated. Instead O-R presumed these characteristics for the economy. Prychitko confirms that he is “not aware of any Austrian who has tried to demonstrate that the presence of ‘radical’ subjectivism and ‘genuine’ uncertainty is *compatible* with fundamental convergence properties toward general equilibrium” (374).

The Keynes-Post Keynesian claim, on the other hand, is that it is possible to demonstrate that an entrepreneurial system, operating

in a creative reality, may not automatically ensure, in either the short or long run, a "convergence" toward a full employment position.

Prychitko, however, then changes the question. Austrians, he claims, are not concerned about demonstrating that the market system ensure full employment coordination of agents' plans. Rather Austrians merely want to show that even in a "world of profound ignorance" a laissez-faire market system leads to "some degree of order" (374). The "degree of order" need not be full employment (or what Prychitko calls "general economic equilibrium"). Since, according to Prychitko (379n9), Post Keynesians (and Keynes?) are using the term "equilibrium" in a "nonstandard" way when they claim the possibility of less than full employment equilibrium, why, he asks, can't others, such as O-R, redefine equilibrium to suit *their* purposes?

Keynes and the Post Keynesians, however, have *not* redefined the concepts of equilibrium or general equilibrium. In his criticism, Prychitko makes the same linguistic error that many mainstream economists do. He conflates the concept of equilibrium with that of market clearing. Alfred Marshall borrowed the equilibrium concept from engineering, where it merely meant a balancing of forces so that nothing changes in the system. For example, the labor market may be in Marshallian equilibrium while it is *uncleared* (i.e., there may be a surplus of unemployed workers at the going wage). Unemployment equilibrium can exist if one can demonstrate that labor market demand and supply offers are unchanged at the existing market price even though the labor market is uncleared. Clearing is a sufficient condition for equilibrium, but it is not a necessary condition.

What mainstream economists call general equilibrium is a situation where all markets—including the labor market—clear simultaneously. Keynes and the Post Keynesians label this situation a full employment equilibrium. Keynes and the Post Keynesians claim they can demonstrate that there is nothing in a laissez-faire system that ensures that the economy automatically always approaches a full employment equilibrium. Intelligent government policies, on the other hand, can ensure such a convergence.

In the Keynes-Post Keynesian view then, economies are not inherently disequilibrating, although I think Prychitko believes otherwise.³⁴ Prychitko therefore hopes that the Austrian analysis of

a “degree of order” in a market system is a useful addendum to the Post Keynesian synthesis. He implicitly challenges Post Keynesians to explain why mass unemployment persists but stabilizes near 7 percent and never reaches horrendously higher rates. Of course, with 20 million unemployed in capitalist Western Europe and an unemployment rate of more than 12 percent, and with unemployment rates of 40 to 50 percent in the market economies of Eastern Europe, many Europeans would be skeptical of Prychitko’s optimism regarding a stable “degree of order” of approximately 7 percent in a free-market system. Even in the United States during the Great Depression, unemployment reached 25 percent. Had Hoover been reelected in 1933 and continued to rely on the market mechanism and sound finance for maintaining an orderly economy, it is not clear that the market system in the United States would have maintained any survivable “degree of order” as unemployment rates might have continued to rise.

Response to Boehm and Farmer

B-F spend a substantial portion of their paper (the appendix) attempting to show that my simplification of Walrasian GE theory is “superficial, incorrect, and outmoded” (407). This esoteric discussion of recent complex GE models has nothing to do with the question of uncertainty and Austrian economics. As for my “sloppy manner” of exposition, it does not seem to bear on the correctness of my critical discussion of the classical axioms and my description of a nonergodic world, especially since B-F concede that I have touched “on sore spots of both Austrian and neoclassical analysis” (407).

Their primary criticism, therefore, reduces to the complaint that I used a “reckless reviewing style” (412) when I set up a simplified version of the basic GE model as a “straw man” (409) and restricted myself to what I believed were the major arguments of O-R.³⁵ Apparently, B-F would have preferred that I use a more esoteric (less sloppy?) jargon and discuss more of the recent complex variations of the basic GE model. Simplification for a book review essay directed to an interdisciplinary audience amounts to sloppiness and superficiality in B-F’s eyes³⁶—even though they seem to accept my fundamental message regarding the role of money in a transmut-

able economic reality. Neither my original review of O-R nor this rejoinder is the place (or contains the space) to engage B-F on the merits or faults of recent variants of Walrasian GE theory vis-à-vis the basic GE model as a benchmark for comprehending either how mainstream theories deal with the effect of uncertainty on the economy, or their axiomatic view of external reality.

B-F do not challenge the correctness of the 1971 Arrow-Hahn demonstration that in an economy with money and contracts where calendar time is important, one cannot prove the existence of a market solution that automatically coordinates all individuals' plans and clears markets simultaneously. B-F never deny that given the presence of money, contracts, and real time, all existence proofs regarding a full employment equilibrium are jeopardized. Instead, B-F indicate that the Arrow-Hahn demonstration has been "completely neglected" (8) in *the mainstream literature*³⁷ since 1971, thereby implying that Arrow and Hahn's presentation must be either wrong and/or unimportant.³⁸ Instead, I believe that this demonstration by Arrow and Hahn, two leading GE experts (one obtaining the Nobel Prize for his contribution to GE theory), has gone *completely unchallenged* (rather than having been "completely neglected") since 1971 because Arrow and Hahn's proof is logically correct and watertight. Accordingly, for mainstream economists who have invested heavily in developing more complex GE models, it is better to "neglect" this devastating finding and go on building models "as if" money, contracts, bankruptcies, time, and so on, did not matter in the real world.

B-F claim that "Davidson fails to differentiate between sufficient conditions for the existence and sufficient conditions for the uniqueness of general equilibrium price vectors" (417). For those interdisciplinary readers who are unfamiliar with the jargon of economic technicians, B-F are saying that since basic GE theory presumes that there *exists* a single (*unique*) set of relative prices that a free market will establish to ensure a full employment equilibrium, I failed to clarify whether I was criticizing the GE presumption of the *uniqueness* of a market-clearing price vector or of the *existence* of such a set of relative prices.

If my exposition was not clear on this matter, let me make my position obvious now. When Keynes insisted that one of the "essential properties" of money (and all other liquid assets) is that it possesses zero elasticity of substitution compared to the products

of industry,³⁹ he specifically rejected the universal applicability of the classical gross substitution axiom. *This classical substitution axiom is a sufficient condition for the existence of the GE market-clearing price vector.*⁴⁰ Accordingly, all existence proofs that rely on market changes in relative market prices to ensure a full employment (simultaneous all-market clearing) outcome are jeopardized if this gross substitution axiom is jettisoned.⁴¹ This does *not* mean that Keynes and the Post Keynesians are claiming that a full employment equilibrium cannot exist. Nevertheless, the zero elasticity of substitution between liquid assets and the products of industry does mean that theorists cannot rely on freely flexible market prices (or freely flexible exchange rates) automatically to bring about a full employment equilibrium. Full employment could, of course, occur *if* private consumption and investment spending decisions just happened to sum to an amount that ensured there was no lack of effective demand in the private sector. Or it could exist if government policies acted as the aggregate demand balance-wheel to ensure that there is never a lack of effective demand at full employment.

I have said nothing about uniqueness or the possibility of multiple equilibria. There are an infinite number of ad hoc constraints, including the making of erroneous decisions, that can be imposed on the basic classical GE model to produce either a unique (or a multiple of) *short run* less-than-full-employment equilibrium (equilibria). These equilibrium models produce what is euphemistically called a short-run “Keynesian-type” unemployment situation.”⁴² The logic of all ad hoc constrained GE models implies that, in the long run, where these constraints disappear, a free market will bring about a full employment equilibrium.⁴³ Keynes and the Post Keynesians deny the applicability of the GE model with or without ad hoc constraints as the primary explanation of unemployment “in the economic society in which we live, with the result that . . . [they are] misleading and disastrous if we attempt to apply [them] . . . to the facts of experience.”⁴⁴

Finally, two minor points that B-F raise. First, they incorrectly accuse me of “donning the mantle of an economist-scientist” (409). Since I reject Samuelson’s ergodic criterion for an economist-scientist, this charge cannot be valid.

Second, B-F approvingly cite Sheila Dow (another member of the board of editors) for preferring a Babylonian economics to a

Cartesian/Euclidean specific axiom-based economics (411). I prefer a Cartesian/Euclidean approach for explaining differences among schools of thought. After all, Keynes wrote that "classical theorists resemble Euclidean geometers in a non-Euclidean world who, discovering that in experience straight lines apparently parallel often meet, rebuke the lines for not keeping straight—as the only remedy for the unfortunate collisions which are occurring. Yet, in truth, there is no remedy except to throw over the axiom of parallels and to work out a non-Euclidean geometry. Something similar is required today in economics."⁴⁵ I think this research program of Keynes's *General Theory* is still as relevant as it was almost a half-century ago.

Perhaps the clearest exposition of my position can be provided by a slight paraphrasing of Reinhold Niebuhr's famous "Serenity Prayer": "God, give us grace to accept with serenity the things that cannot be changed [immutable realities], courage to change the things which should be changed [transmutable realities], and the wisdom to distinguish the one from the other."

NOTES

1. J. M. Keynes, *The General Theory*, in *The Collected Writings of John Maynard Keynes*, vol. 13, ed. Don Moggridge (London: Macmillan, 1973), 470.
2. Gerald P. O'Driscoll and Mario J. Rizzo, *The Economics of Time and Ignorance* (New York: Blackwell, 1985).
3. Subjectivists, such as Milton Friedman, would argue that even if agents do not calculate probabilities, they act "as if" they did.
4. Have agents spent any resources to obtain the incomplete knowledge that Austrians believe agents already possess?
5. O'Driscoll and Rizzo, 180.
6. If reality is transmutable, then it would just be happenstance that entrepreneurs, in the long run, made no persistent systematic errors.
7. J. M. Keynes, "The General Theory of Employment" [1937], reprinted in *The Collected Writings of John Maynard Keynes*, vol. 14, ed. D. Moggridge (London: Macmillan, 1973), 114.
8. Fundamental differences in the policy implications of various macroeconomic theories are the result of distinctions embedded in each theory regarding the conceptual basis of economic external reality.
9. Tony Lawson, "Probability and Uncertainty in Economic Analysis," *Journal of Post Keynesian Economics* 11 (Fall 1988): 38-65.
10. Asymmetric information theory is a hybrid of type 1 and type 2 immutable systems. These models assume that some agents have already adapted their

subjective expectations to the objective reality while other agents have not been able to do so. When O-R claim that "not all individuals know the same things," they are utilizing an asymmetric information framework; see O'Driscoll and Rizzo, 22.

11. O'Driscoll and Rizzo, 38.
12. O'Driscoll and Rizzo assume that a "vague Darwinian process" (40) will occur.
13. Even if, in the short run, these agents do not "know" they are making optimal choices.
14. Paul Davidson, "Reviving Keynes's Revolution," *Journal of Post Keynesian Economics* 6 (Summer 1984): 561-86; Paul Davidson, *Post Keynesian Macroeconomic Theory* (Brookfield, Vt.: Edward Elgar, 1994).
15. Paul A. Samuelson, "Classical and Neoclassical Theory," in Robert W. Clower, ed., *Monetary Theory* (London: Penguin, 1969), 184-85.
16. Frank H. Knight, *Risk, Uncertainty, and Profit* (Chicago: University of Chicago Press, 1971), 233.
17. *Ibid.*, 198.
18. *Ibid.*, 210.
19. Paul Davidson, "The Economics of Ignorance or Ignorance of Economics?" *CRITICAL REVIEW* 3, nos. 3-4 (Summer-Fall 1989): 467-87.
20. John Hicks, *Economic Perspectives* (Oxford: Clarendon Press, 1977), vii.
21. J. M. Keynes, *The General Theory*, ch. 12. It is here that Shackle's concept of the crucial experiment is relevant. For Shackle, an agent engages in such an experiment when "the person concerned cannot exclude from his mind the possibility that the very act of performing the experiment may destroy forever the circumstances in which" the choice was made. (G.L.S. Shackle, *Uncertainty in Economics* [Cambridge: Cambridge University Press, 1955], 6.)
22. Keynes, *The General Theory*, 377-79.
23. Moreover, at full employment there is a greater incentive for entrepreneurs to search for more productive means of producing output, thereby yielding an additional Schumpeterian efficiency as a byproduct of a drive for Keynesian efficiency.
24. Samuelson's criterion is based on a nineteenth-century conception of physics as the ideal science. Since twentieth-century physicists have discovered nonergodic phenomena, modern physics would not meet Samuelson's definition of science.
25. Keynes, *The General Theory*, 163.
26. *Ibid.*, 152.
27. Roy Radner, "Problems in the Theory of Markets under Uncertainty," *American Economic Review Papers and Proceedings* 60 (May 1987): 451.
28. I would be remiss, however, if I did not call attention to two articles by Carvalho: Fernando Carvalho, "On The Concept of Time in Shackle and Sraffian Economics," *Journal of Post Keynesian Economics* 6 (Winter 1983-84): 265-80; and "Alternative Analysis of Short and Long Run in Post

Keynesian Economics" *Journal of Post Keynesian Economics* 7 (Winter 1984-85): 214-34. In the 1983-84 article (272), Carvalho argues that Shackle misses an essential ingredient of the Keynes-Post Keynesian approach, namely, that the institution of forward money contracts creates a structural orderliness (stability) over the period of production in a monetary economy. In both articles, Carvalho shows that the Sraffian (e.g., Garagnani, Eatwell) approach, with its emphasis on either a static or a moving center of gravity in response to "persistent forces," represents a significant difference from the uncertainty approach of the Keynes-Post Keynesian system. The Sraffian "vision" assumes the existence of long-run persistent (i.e., immutable) forces.

29. Paul Davidson, *International Money and The Real World* (London: Macmillan, 1982).
30. Some may be shocked to see Kregel, a former student, listed in the neo-Keynesian school along with the Sraffians. This was because when this was written (in 1980), I had attended a private half-day-long meeting in Rome called by Paolo Sylos-Labini to see if a single Post Keynesian vision could be crafted from these diverse views. To my recollection the only others at the meeting were Garagnani, Roncaglia, Kregel, Sylos-Labini, and myself. The main area of dispute was the existence of centers of gravity. At that meeting, Kregel accepted the Garagnani position that such centers existed. I believe that in the years that followed Kregel abandoned this view.
31. In 1974, Hicks and I met at an International Economics Association conference in S'Agora, Spain, on "the microfoundations of macroeconomics." Also attending were members of what are now called the neo-Ricardian (Sraffian), American Keynesian (e.g., Stiglitz), and Monetarist (e.g., Parkin) schools. The conference was a failure in that the representatives of the diverse schools could not agree on *the* microfoundation. At the end of the week's conference, Hicks confided that he came closer to my views than any of the other participants. This started up a correspondence between us punctuated by meetings at the Athenaeum in London and Hicks's country house near Oxford whenever I was in England. Over this extended period our views tended to converge. By the winter of 1980-81 I published Hicks's recantation of IS-LM analysis, which he had originated and which, although augmented since 1937, still remains the basic structure of most mainstream macroeconomic models.
32. O'Driscoll and Rizzo, 180.
33. *Ibid.*, 40.
34. B-F also argue, incorrectly in my view, that Post Keynesianism draws attention to the "endemic instability" in the system. Keynes, however, indicated that a state of involuntary unemployment equilibrium "may be called long-period employment" to emphasize that a market system could settle down to such a stable state (see Keynes, *The General Theory*, 48). Inherent instability is not necessarily one of capitalism's inherent faults. Its

- “outstanding faults . . . are its inability to provide full employment and its arbitrary and inequitable distribution of wealth and incomes” (*Ibid.*, 372).
35. What I perceive as the main body of arguments in O-R, B-F think is another “straw man.”
 36. Can it be that B-F’s insistence on using esoteric professional jargon, in an interdisciplinary journal which expressly warns prospective authors that “technical jargon and topics of interest only to experts should be avoided,” is an attempt to hide from the readers who are not economic “experts” the transparently vacuous nature of the classical Emperor’s new clothes?
 37. But not in the Post Keynesian literature!
 38. Arrow and Hahn (356–61) demonstrate that if any debtors go bankrupt, then it is not possible to prove the existence of market forces that can coordinate all agents’ plans. Since, even in prosperous times, every year there are many households and firms that go into bankruptcy in the market economy in which we live, a reasonable person might question the applicability of any model which assumes away “Hahn’s non-existence example” by insisting on ignoring the possibility of bankruptcies by presuming the existence of a market system that will automatically coordinate all agents’ plans, at least in the long run.
 39. Keynes, *The General Theory*, 231, 235–36, and esp. 241n1.
 40. Since gross substitutability is a *sufficient* condition for the existence of a full employment equilibrium, then if one presumes gross substitution one is assuming (*not proving*) the existence of a set of relative prices that will clear all markets simultaneously. Consequently, one is assuming that a free market can automatically achieve a full employment equilibrium and coordinate the plans of all the agents in the economy. Since gross substitution is *not a necessary* condition for a full employment market-clearing set of relative prices, however, the absence of the gross substitution axiom simply implies that it is not possible to demonstrate that a free market can automatically achieve a full employment equilibrium. It does not mean that full employment can never be achieved.
 41. I first published my arguments about the importance of throwing out the gross substitution axiom in my “A Post Keynesian View of Milton Friedman’s Monetary Framework,” *Journal of Political Economy* 80 (September–October 1972): 883–905 (reprinted with an addendum in *Milton Friedman’s Monetary Framework: A Debate With His Critics*, ed. Robert J. Gordon [Chicago: Chicago University Press, 1974]). It is pleasing to find that B-F have recognized that since 1972 some mainstream theorists have developed models that do not possess short-run ubiquitous gross substitution as an attribute. Many of these theories have distracted attention from the question of whether a full employment equilibria can automatically be achieved by market forces by presuming the *existence* of a multiplicity of short-run equilibria achieved by some ad hoc constraint on the basic GE model. What these ad hoc mainstream theorists have yet to discover is that Keynes came to the “essential” elasticity-properties conclusion as a logical

necessity in the development of his long-run nonneutral money model of the operation of an entrepreneurial economy under uncertainty. These modern mainstream macroeconomic models, on the other hand, justify the lack of gross substitution by asserting some ad hoc constraint (e.g., rigid money wages, asymmetric information, market coordination failures) on a long-run GE model that still has microfoundations embedded in the classical axioms of an immutable reality and the long-run neutrality of money—axioms that, as I have indicated, were long since discarded by Keynes and the Post Keynesians.

42. These models may explain “Keynesian-type” unemployment, but they do not explain Keynes’s long-run underemployment equilibrium!
43. For example, in explaining short-run “Keynesian-type” unemployment due to “extraneous uncertainty” induced by sunspots, Costas Azariadis, “Self-Fulfilling Prophecies,” *Journal of Economic Theory* 25 (1981): 380, notes that this uncertainty “disappears in the long run.” Jean-Michel Grandmont and Pierre Lagrange in “Nonlinear Economic Dynamics: Introduction,” *Journal of Economic Theory* 40 (1986): 9, note the conditions where “there is generally a large multiplicity of intertemporal equilibria with self fulfilling expectations”: “while [agents] are learning the dynamic laws of their environment, during the period of transition of the economy towards an hypothetical long run equilibrium . . . [where] forecasting errors vanish eventually.”
44. Keynes, *The General Theory*, 3.
45. *Ibid.*, 16.

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